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A POSSIBLE ORIGIN FOR SOME SPIRAL NEBULAE

By George F. Becker
UNITED STATES GEOLOGICAL SURVEY, WASHINGTON
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In speculations on the evolution of nebulae it has become the fashion to postulate an initial spheroid consisting exclusively of elastic fluids, this assumption lending itself most readily to exact reasoning and computation.

Kant, Herschel, and Laplace, however, did not assume gaseous nebulae. In a paper on Kant as a natural philosopher, printed in 1898, I gave an outline of his hypothesis including the following passage:[1]

> Tendencies to motions in all directions, excepting in one resultant plane, are suppressed by mutual interferences of the free particles. Most of the material accumulates at the center, in the sun, but a wide, thin disc of heterogeneous matter remains. This disc consists of discrete particles each of which has acquired such a velocity and direction as to maintain the appropriate orbital motion. . . . . Mutual attraction and adhesion, beginning at relatively massive particles, cause the agglomeration of the particles in any zone or ring to single planets or to groups of planetary bodies.

From this and other passages it appears that to this very original thinker solid particles were the most essential components of nebulae.

William Herschel's nebular hypothesis was founded upon induction and observations. Nowhere in his own works do I find so graphic a résumé as the following which Laplace gave in the *Système du Monde:*[2]

> Herschel, while observing the nebulae by means of his powerful telescopes has followed the progress of their condensation, not in a single one, for this progress could not become sensible to us until centuries had passed, but in the aggregate; as in a vast forest one traces the growth of the trees among the individuals of diverse ages which it contains. He first observed nebulous

matter spread out in diverse bodies in different parts of the heavens of which it occupies a great area. He saw in some of these bodies the nebulous matter slightly condensed about one or several slightly luminous nuclei. In other nebulae, these nuclei shone more brightly relatively to the nebulosity which surrounded them. The atmosphere of each nucleus was about to segregate by a final condensation: multiple nebulae resulted formed of brilliant nuclei very close together and each surrounded by an atmosphere: occasionally the nebulous matter condensing uniformly produced the nebulae called *planetary*. Finally a greater degree of condensation transformed all of these nebulae into stars. The nebulae classified according to this philosophic view indicate with extreme probability their future transformation into stars and the former nebulous state of the stars now in existence.

It will be observed that nebulae with a single nucleus are mentioned only as special cases of multinuclear and, therefore, heterogeneous nebulae. Herschel himself made repeated references to the character of nebulous matter. For him it was anything which could shine, and while he was unable to come to definite conclusions, he regarded it as probable that it comprised matter in all three states.

Laplace's information as to nebulae was derived almost wholly from Herschel; so far as I can ascertain he made no telescopic observations of his own on these bodies, and his ideas of the constitution of nebulae seem to have been identical with those of the great English observer. Both in the passage quoted above and in his famous note on the nebular hypothesis Laplace referred to atmospheres, but they were cloudy or dusty atmospheres not inconsistent with the presence of multiple nuclei; they were characterized by a 'numberless variety of densities' and seem to have resembled volcanic clouds. In other essays, such as that on the barometer, Laplace showed his complete mastery of Boyle's and Gay Lussac's laws and in these memoirs he frequently employs the word gas. In his note on the nebular hypothesis he never once uses this word and the very first application he made of his theory was to offer an explanation of the genesis of the Pleiades. In his time, the presence of nebulous matter in this cluster was unknown, but the photographic plate shows that it is embedded in one of the most astonishing of the great nebular masses. Laplace's selection of the Pleiades was thus a striking example of his prescience.

Much as more modern science has contributed to knowledge of the nebulae, the question of their constitution is still unsettled. That some of them contain luminous gas (helium, hydrogen, nebulium) was shown by Huggins 50 years ago, but many nebulae and among them the spirals show continuous spectra. The continuity might be due to

gases at high pressures; but that gases should assume so complex a configuration as that of a spiral is difficult to understand while the continuous spectrum is more characteristic of solids than of gases, and Mr. Slipher has adduced evidence that some nebulous matter shines by reflected light. The extreme tenuity and presumably low temperature of nebulae increase the difficulty of a purely gaseous hypothesis, which is further contradicted by the abundance of multinuclear nebulae.

I infer that it is perfectly legitimate to speculate on celestial clouds composed of matter in more than one and perhaps in all three states.

Provided that there is a limit to the expansion of gas, as G. W. Hill believed, a gaseous spheroid may assume a figure of equilibrium and this is the favorite postulate among modern cosmogonists. Some of the more regular nebulae lend color to it; but the forms of the celestial clouds are as varied as those of our atmosphere and might in large part be similarly classified. Surely some of the elongated nebulae, resembling cirrus or cirro-cumulus terrestrial clouds, must eventually develop into more highly organized forms; or, inversely, some well-developed nebulae may have originated from whisps of nebulous matter, such as abound in the sky.

It is not possible at present to assign a definite origin for these nebulous streamers. The hypothesis that the whole galaxy was nebulous at a certain epoch leads to a dilemma as was long since pointed out.[3] In 1900 Arrhenius put forward his theory that energy is degraded in the solar state but raised to a higher level in the nebular state,[4] and some such 'third law' of thermodynamics seems logically inevitable. But whether or not there is a regenerative process at work among the heavenly bodies, there are at least inequalities of action and preferential movements (so suggestively discussed by Kapteyn) which would almost inevitably lead to a carding or filamentation of nebulous masses.

It has always been impossible to suppose that nebulae were devoid of internal motion, but such movements were first demonstrated in 1914 by Messrs. Buisson, Fabry, and Bourget[5] for the Orion nebula. Their result has been confirmed by Messrs. Frost and Maney,[6] who point out that this nebula must be considered as a mass writhing and seething in irregular contortions. A similar statement must be true of all nebulae.

Consider then an elongated nebula composed of heterogeneous matter and bounded by a surface which may be very irregular but not so irregular that any part of its longest principal axis falls outside of the mass. Such a nebula would bear some resemblance to a staff or baculum which may be provided with knots, knobs, and excrescences. For the sake of brevity I will characterize such a nebula as bacular. A bacu-

lum then must be possessed of energy and moment of momentum, but I shall suppose its axis of rotation to stand at an angle to its greatest principal axis of inertia.

Such a bacular nebula would pass through some of the phases familiar to readers of cosmogony; energy would be dissipated by collisions; a part of the material would subside towards a central nucleus or several excentric nuclei; excepting for some outlying cometary detritus, the mass would tend to flatten in the invariable plane; and finally orbits of great eccentricity would or might be reduced by collisions to an approximately circular form, while of course the moment of momentum would remain nearly or quite constant. The flattening of such a nebula and its rotation about the centre of inertia of the whole system are so nearly independent that the principle of superposition is applicable; the mass may be supposed to subside into its invariable plane without flexure of the axis of the baculum, and thereafter to rotate about a line perpendicular to this axis.

The question then arises how the axis of a bacular nebula would be distorted compatibly with Kepler's third law if the orbits of the component particles were sensibly circular, or if eccentricities are neglected, only a first approximation being sought.

In its simplest form Kepler's third law expresses the equality between the attraction of a heavy point on a particle moving in a circular orbit and the centrifugal force, or normal acceleration, of the particle. If ω is the angular velocity of the particle, r its distance, and M the mass of the heavy point,

$$\frac{M}{r^2} = \omega^2 r.$$

Here M may mean the mass of a system concentrated at its centre of inertia and it is easy to see that r may be replaced by the mean distance a, of a particle moving in an elliptical orbit.

The law so expressed would be valid for the solar system, so far as two bodies are concerned, were it practicable to take the centre of inertia of that system as an origin; most astronomical problems, however, involve not absolute but relative motions, and Kepler's law must then be modified by substituting $M+m$ for $1/M$.[7] For small planets m can be neglected because the sun's centre is then almost exactly at the centre of inertia of the system.

If a system consisted of a single nucleus and a great number of minute secondary particles which, at a given epoch, were arranged on an axis,

and thereafter for a time τ described circular orbits, then, putting $\tau\omega = \varphi$ and $a = r$ the particles would be found on a curve

$$r^3\varphi^2 = \text{constant}.$$

This is a spiral with a single well-defined point, which is a point of inflection. If r_0, φ_0 are the coordinates of this point it is easy to show that $\varphi_0^2 = 2/9$ so that

$$r^3\varphi^2 = 2r_0^3/9.$$

If the axial row of particles extended on both sides of the nucleus, the spiral would have two branches, each approaching the axis asymptotically from opposite sides. The length of any portion of the axis would be much extended by conversion into the spiral, the extension at any point being $d\,s/\,d\,r$, which, at the point of inflection is $\sqrt{3/2}$, while closer to the centre it is still larger.

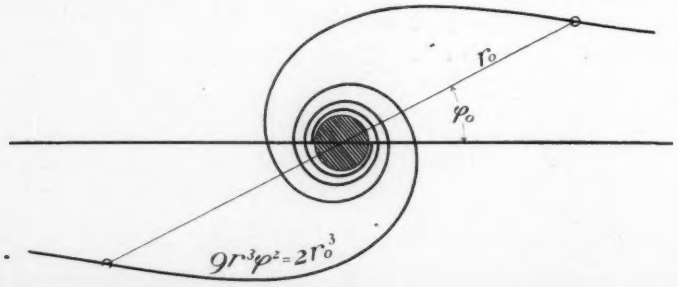
For secondary bodies of finite mass the problem presented would be one of n mutually perturbing bodies, but the principles of centre of inertia, energy, and moment of momentum would remain in force and parabolic velocities could not be attained. It would appear, therefore, that even in a system of finite masses, axially arranged, angular velocities must diminish with distance from the centre of inertia and be infinitesimal at an infinite distance. Then the axis must be distorted into a more or less irregular spiral which is asymptotic to the axis and must, therefore, have a point of inflection. These conditions would be satisfied by any curve of the form $r\varphi^{n^2} = \text{constant}$, $n^2 < 1$, for these spirals have a point of inflection at $\varphi_0 = n\sqrt{1-n^2}$, the maximum value of which is $1/2$.

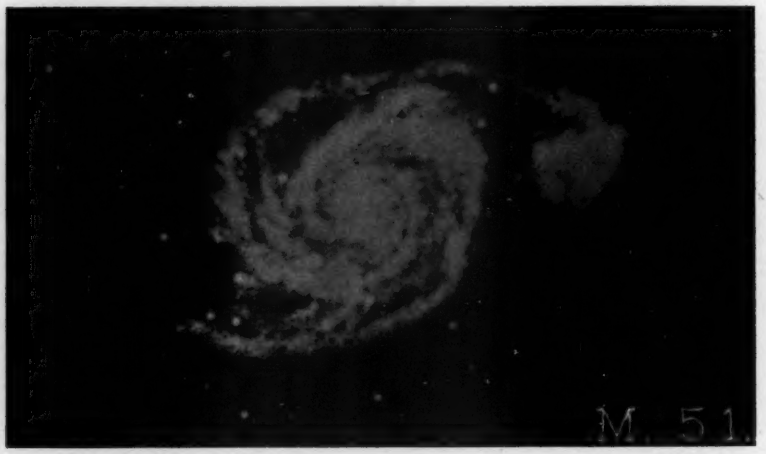
Possibly perturbations might bring about approximations to some of these curves, but since perturbations are necessarily excluded from consideration, the only curve with any standing is that in which $n^2 = 2/3$.

Supposing, then, that the initial configuration of the system were an axial line passing through the centre of inertia and that the orbits were circular, the configuration after the lapse of a given time would vary from the spiral $r^3\varphi^2 = \text{constant}$ only as a result of perturbations and this spiral must represent, to a first approximation, the axis of such spiral nebulae as have been developed from substantially rectilinear nebulous streamers, while in some other cases it will represent displacements from an initial configuration.

The diagram shows the form of the spiral excepting that near the center where it would be almost impossible to plot the curves, a nucleus is substituted. Both branches are prolonged somewhat beyond the

points of inflection. Of course, the curve in the diagram represents only the axis of the baculum. Below the diagram is shown the Whirlpool nebula[8] for comparison, and evidently a diagram could be drawn representing the mathematical spiral clothed, so to speak, with irregularly disposed nebulous material which would add to its resemblance to the photograph.





As noted above, even at the point of inflection the elongation of the original axis is in the ratio of $\sqrt{3/2}$ or 1.22, while for points nearer the origin it is more than this. Unless, therefore, the original baculum were of extraordinary uniformity, this stretching combined with local partial attractions would break up the spiral into short curved fragments. Between the point of inflection and the origin such fragments, would present their concave sides to the origin. If any such fragment were to condense to a spheroid, the centre of the spheroid would be

close to the centre of inertia of the arc corresponding to the fragment; therefore the condensed mass would be nearer to the centre of attraction than in the uncondensed state and consequently the rotation of the spheroid would be positive. Beyond the point of inflection these conditions would be reversed so that condensing short arcs of the nebulous spiral would retreat and show retrograde rotation. In systems thus developed, then, positive rotation would characterize all but the outermost portions where retrograde rotation would prevail.

A bacular nebula so short or so old that its outermost portions had completed two or three revolutions round the centre of attraction would be almost or quite indistinguishable from a circular disc, marked by furrows and closely resembling Laplace's nebula after the development of rings. From this epoch onwards all the valid conclusions drawn from Laplace's great hypothesis would be applicable to the nebula under consideration, and in particular the splendid investigations of Roche, Hill, and George Darwin on unstable orbits.

So far as I can see, a baculum of very uniform composition might develop into a star without planets, though possibly attended by a disc reflecting zodiacal light; while a very heterogeneous baculum might yield a system as complex as the Pleiades. There seems to me no reason why even the most symmetrical planetary nebulae might not have been evolved from bacula closely coiled. On the other hand, a nebula much less symmetrical than a baculum as I have defined it, can be similarly discussed. Suppose the axis of figure of an elongated nebula to be any arbitrary curve of small curvature; then the equation $r^3\varphi^2 = 2r_0^3/9$ does not represent the distorted axis but does give the displacement of points on the assumed curve. Or inversely, comparison of a well-defined spiral nebula with the spiral represented by the equation should show to what simplest form the nebula could be reduced by reversing the orbital velocities. Again, if the axis of figure of the filamentous nebula were coincident with an arc of a circle, its centre of inertia might lie outside of the nebula; and in such a case, after the extremities of the nebula had revolved about the centre of inertia a few times, the figure would be indistinguishable from an empty ring; but if a portion of the mass were arrested by collisions, the ring would show a central nucleus as does the ring nebula in Lyra.

Superficial comparison of the diagram given in this paper with photographs of the Whirlpool and other nebulae is not unfavorable to the hypothesis here developed, and it seems to me that to the first order of approximation bacular nebulae must ultimately be converted into the spiral here discussed. But the geometry of this spiral is so well characterized

that astronomical observers will probably be able to make a definite decision as to whether or not any of the existing spiral nebulae have developed from bacula or from less regular forms by substantially the same method. Till that is decided further discussion would be superfluous. It is possible that there are well developed spiral nebulae in which mutual interference has done little or nothing to reduce orbits to ellipses of small eccentricity. To such nebulae the analysis here indicated has no application, nor could their motions be formulated in the present state of science.[9]

[1] *Amer. J. Sci.*, Ser. 4, **5**, 102 (1898).
[2] Chapter VI, p. 482, of the edition of 1835.
[3] *Amer. J. Sci.*, Ser. 4, **5**, 106–107 (1898).
[4] See preface to *Worlds in the Making*, 1908.
[5] *Paris, C. R. Acad. Sci.*, **158**, 1017 (1914); and *Astrophys. J.*, **40**, 241 (1914).
[6] *Pop. Astr.*, **23**, 485 (1915).
[7] A. G. Webster, *Dynamics of Particles*, etc., 2d ed., 1912, p. 317.
[8] From a photograph taken at the Yerkes Observatory.
[9] Just in time for a reference, I have met with an interesting paper by Mr. E. J. Wilczynski, *Astrophys. J.*, **4**, 97, (1896), who pointed out that, if circular orbits are assumed, every long streak of nebulous matter must eventually be converted into a spiral as a consequence of Kepler's third law. The equation of this curve, he says, it would be easy to deduce.

A PECULIAR CLAY FROM NEAR THE CITY OF MEXICO

By E. W. Hilgard

UNIVERSITY OF CALIFORNIA

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In 1912 Dr. George W. Shaw, then connected with the College of Agriculture of the University of California, was requested to visit the Hacienda Santa Lucia, in the neighborhood of the City of Mexico, in order to give advice for the reclamation of certain tracts of land which were supposed to be afflicted with 'alkali,' and which had resisted the usual methods for rendering them productive. Dr. Shaw brought back samples of the soils from these alkali spots, which were as usual depressed in the middle, but found in them no excessive amounts of carbonate of soda, and that the sulphate (Glaubers salt) was mainly present in fractions of one per cent, not enough to injure vegetation.

In an attempt to leach one sample of its soluble salts he found that on pouring water on a few grams placed in a 50 cc. cylinder the substance swelled very rapidly, and over-night actually filled the cylinder to the top, making a semifluid slush. As I had never seen the like before, I undertook to investigate it.

The material, looking like an ordinary dark gray clay, adhered strongly to the tongue, and with a little water became very plastic. Unfortunately the main sample collected by Dr. Shaw came to the laboratory unlabeled and was by mistake thrown away by an assistant. A small sample of about 24 grams, however, was left; and on this the entire investigation had to be made. About half of this sample was boiled in distilled water for 24 hours for sedimentation, as there were some coarse particles, apparently of talcose schist. A reference to geological descriptions of the basin of Mexico, in which serpentines and talcose rocks are prevalent, seemed to confirm this conclusion.

The suspension did not appear to be very uniform even after protracted boiling, and under the microscope showed a multitude of dark rounded particles, very uniformly distributed through a colloidal medium of faintly yellowish tint, which when colored with a solution of 'malachite green' merely showed the fine discrete particles in larger numbers and greater fineness.

All attempts to free the colloidal ingredients from the visibly discrete particles by sedimentation proved futile. The suspension was readily coagulated and precipitated, apparently unchanged, by a solution of sodium chloride. On washing by decantation the suspension was again readily made, the microscopic character also remaining the same.

As the original material effervesced somewhat with hydrochloric acid it was conjectured that the minute grains might be earth carbonates; but the addition of drops of tenth-normal acid was slow in producing coagulation, doubtless on account of the neutralization of the acid by the earth carbonates. The filtrate gave reactions of calcium and more strongly of magnesium; yet the coagulum itself seemed to contain as many discrete particles as before, showing that the earth carbonates were very finely distributed, and not visible.

For the determination of the specific gravity some pure selected fragments weighing 1.073 grams were dried in a 25 cc. bottle. Filling the bottle, 0.481 gram of water was found to have been displaced, making the specific gravity about 2.25, nearly that of kaolinite clays. The substance was left in the gravity bottle until, with the aid of slight heating, it had absorbed the maximum amount of water, leaving a clear remainder above it. The water poured off measured 14 cc., showing that 11 cc. had been taken up by the clay, forming a coherent, gelatinous-looking mass, and giving an increase of bulk equal to about 25 times the volume of the clay. The mass was then evaporated to dryness in an air bath, with suction, at about 50°C., in order to avoid a possible molecular change; the bottle then was refilled with water. The reabsorption took place

more slowly than with the crude substance, and although aided by heating to 100° for three days, did not recover the full volume of 11 cc.

A repetition of the experiment with 1.70 grams of dry clay which had been precipitated from diffusion by ammonium carbonate, gave an increase of volume to 32 times that of the clay.

A parallel experiment with some ordinary plastic clays showed an increase of only two to two and a half times the original bulk.

Chemical Composition of the Clay.—On leaching the original substance with water, it was found to contain 2.60 % of soluble salts, of which 1.74 was sodium sulphate, 0.74 sodium carbonate, and 0.12 sodium chloride. Black rings from the evaporation of the alkali solution were reported to exist in the depressed spots, as is usual in the case of 'black alkali.'

In attempting to obtain for analysis a sample free from carbonates, it was found that silica went into solution even when only cold tenth-normal acid was used. It was therefore necessary to analyze the entire mass. On ignition the loss was 19.6 %; the ignited material decrepitated when wetted, but did not swell.

Two analyses of the original material were made; one by acid digestion for five days; another, after ignition, by fusion with alkali carbonate. Unfortunately the material at command was too scanty to permit the use of more than one gram and 1.7 grams for these analyses, respectively, the results being as follows:

Partial Analyses of Clay from Mexico.

	Acid digestion.	Fusion with carbonate of soda. (Ignited substance)
Insoluble	1.83%	0.00
Soluble silica	43.00	51.43
Lime	9.06	8.97
Magnesia	17.11	27.07
Ferric oxid	1.76	10.63
Alumina	3.48	
Ignition loss (water, carbonic acid and organic matter)	19.60	0.00
Soluble salts	2.60	
Total	98.44	98.10

The above data, deficient as they are, show clearly a totally different composition from any 'clay' on record. The alumina present is far below any reasonably assumable compound with the soluble silica; the predominant base being evidently magnesia, and that greatly in excess of the lime present. There is thus an apparent relationship to the saponite or sepiolite group of minerals, but the extremely ready decomposition by acids (even by acetic acid in dilute solution) differentiates

the material pointedly from that group, even apart from the exceptionally high absorptive power for water. I believe that what has been shown is sufficient to characterize a new kind of clay with a predominant magnesium instead of aluminum base, for which I suggest the name of *Lucianite*, from the locality where it was found. A better name, Auxite, referring to the exceptional increase in bulk, is too similar in sound to Augite to be desirable.

Wishing to obtain, if possible, additional material from the Mexican locality, I wrote to Don Jose G. Aguilera, the Director of the Instituto Geologico de Mexico, at the City of Mexico, transmitting also a blueprint map on which Dr. Shaw had marked the several localities from which he took samples. Dr. Aguilera very promptly and courteously responded to the request, despite the disturbed condition of the country (in 1912). Dr. Aguilera also sent statements of the analyses of clay materials from the foundations of the Legislative Palace, when building in 1904. These also show a large predominance of magnesia in the substrata of Mexico City.

The samples received, all from low 'alkali spots' in the Hacienda, resembled in general aspect the material brought by Dr. Shaw, but contained more coarse materials, among which talcose schist and black eruptive rock were readily discernable. The fine portions all showed exceptionally high expansion when wetted, one going as high as 12 times the bulk of the raw, dry mass. It was then attempted to obtain from this sample by sedimentation a clay of higher absorptive power. But the highest result so obtained was only 16 times the original bulk. Qualitative tests of this material showed again a large proportion of calcium and magnesium carbonates, with only small amounts of alumina and ferric oxide. The microscopic characters were the same as before noted in the original material, and there was the same easy decomposition of the silicate present with even weak acids, which stands in the way of obtaining a substance of definite and constant composition, free from earth carbonates.

As regards the reclamation of these alkali spots it is obvious that it cannot be accomplished by liming, as a large amount of calcium carbonate is already present and yet does not prevent the collapse of the colloidal ingredient in drying. The depressed spots probably become nearly level during the rainy season by the expansion of the colloidal ingredient. It seems as though acid treatment, instead of that with lime, were the only possible effective agent.

Circumstances have prevented a further prosecution of this investigation, which will be resumed whenever possible. Meanwhile it may be

fruitful to suggest the possible practical use of these magnesian clays in cases in which a tight tamping is necessary where water has access, as in shutting off crevices in bored wells when, as in those yielding oil, undesirable flows of water interfere, and where tamping with ordinary clays fails to tighten sufficiently and permanently. Other uses for such a prodigiously swelling material readily suggest themselves.

STUDIES OF MAGNITUDES IN STAR CLUSTERS, I. ON THE ABSORPTION OF LIGHT IN SPACE

By Harlow Shapley

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

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All researches on the structure of the stellar universe must take into account the probable and possible effects of the scattering or obstruction of light in its passage through interstellar space. It is recognized, for instance, that if the loss due to absorption, or scattering, by the free molecules of matter in space totals as much as a millionth of one per cent of the visual light in a journey of a hundred million miles, then, assuming the effect proportional to the distance, every star 3500 light-years away would be observed about two magnitudes fainter than its true brightness. As a consequence any uncertainty in the coefficient of scattering, especially if it is large as cited above, is very serious in studies of the distance of the faint stars and particularly in considerations of the stellar densities in various parts of the galactic system. In fact, the hypothesis that light extinction is imperceptible is prerequisite to the conclusion that the stellar universe is finite in extent.

Because of the importance of the subject several extensive investigations have been undertaken in recent years for the purpose of determining the amount of absorption. It is generally assumed that if any dimming of a star occurs it will be apparent as selective molecular scattering, which varies as the inverse fourth power of the wave length of the light.

As the effect for blue light would be about double that for yellow, an obvious method of detecting and measuring selective absorption is through the study of the colors of faint and distant stars. By means of measures of color indices various investigators have found that the fainter stars are on the average redder than the brighter ones, and as the fainter stars are also on the average, the more distant, this excess of redness might be and often is accepted as an indication of space absorption. There are, however, other possible interpretations, including the effect of

intrinsic luminosity on color and the possibility of the predominance of redder spectral types among the faint stars and the more distant ones.

In all previous special studies of the problem, the chief difficulties have been the uncertainty of the distances (so few of which have been directly measured) and their relatively small values. For a given spectral type the observed color excess of the more distant stars is also small. Consequently, though positive values for the absorption coefficient have always been derived, they have in no case been considered as definitely final proof of the existence of an appreciable space absorption. The four most recent determinations (all of which depend upon studies of the colors of relatively nearby stars) give the following values for the increase of the color index for each unit of distance (32.6 light-years):

Observer	King	Kapteyn	Jones	Van Rhijn
Absorption coefficient	0.019	0.0031	0.0047	0.0015 mag.

Since the color effect is cumulative, an increase in the length of the base line increases greatly the accuracy of a measurement of absorption. If instead of using stars in our immediate stellar system, we extend the study of colors to much more distant objects, then we can readily decide whether light scattering is to play an important part in stellar investigations. The advantage of great distances is taken in the present study of globular clusters, and already it is possible to make a definite contribution to the problem of the existence of light scattering.

At the beginning of a systematic study of magnitudes and colors of stars in clusters, the great globular system in Hercules, Messier 13, has been observed photographically, both with ordinary plates sensitive to blue and violet light and with isochromatic plates which respond chiefly to the most effective visual light when the shorter wave-lengths are cut off by means of a yellow color filter. Photographic and photovisual magnitudes for about 1300 stars have been determined. The color indices thus secured are compared with those for nearby stars with identical spectra (whenever the spectra of the cluster stars can be directly observed), and the excess of redness for the cluster stars indicates the amount of space absorption. When the spectra can not be directly observed the color indices in the cluster still may serve to solve the problem. For, if the stars in the distant cluster are even approximately similar to those near the sun, the absolute values of the color indices will tell at once whether there is practically no absorption or whether there is a really appreciable amount.

The detailed discussion of the magnitudes in Messier 13, which will appear as a *Contribution from the Mount Wilson Solar Observatory*, will

include a consideration of the reliability of the magnitudes and color indices and an extended analysis of the observational data. For the present communication a part of the results bearing on the absorption of light in space are shown in a condensed form in the following table. The magnitudes of the stars in the denser regions of the cluster, within 2′ of the center, are omitted, as none of the polar standards upon which the determination of the magnitude is based falls within that area, and consequently no control of the possible systematic errors of a photo-

Frequency of Colors in Messier 13—Number of Stars Tabulated

DISTANCE FROM CENTER OF CLUSTER	COLOR CLASS									
	b0 to b5	b5 to a0	a0 to a5	a5 to f0	f0 to f5	f5 to g0	g0 to g5	g5 to k0	k0 to k5	k5 to m0
2′ to 3′	7	39	10	16	20	50	50	17	5	1
3′ to 5′	8	20	21	7	23	59	44	9	4	2
>5′	1	11	5	4	7	26	21	7	0	1
Totals.....	16	70	36	27	50	135	115	33	9	4

graphic nature is assured. The color class has been defined in the September, 1915, number of these PROCEEDINGS; in brief we may say that, under the average conditions of luminosity and distance that obtain for the brighter nearby stars, color class and spectral class are practically identical, but if considerable absorption exists the two do not correspond for more distant objects, redder color being associated with bluer spectrum.

The most remarkable feature of the distribution of color class exhibited in the table is not the relative paucity of *a*'s and early *f*'s, nor the great range in color, but rather the highly significant fact that there are any negative color indices (*b*0–*b*9). Of the 495 stars more than 17 % are of color class *b*, and of these one-fifth are bluer than represented by *b*5. Moreover, we find no excessively large color indices, and none unusually small. A comparison of these colors with those derived by Parkhurst for bright stars near the north pole shows an entirely analagous distribution among the various classes. There seems to be no reason to doubt the present result, and we are left with the conviction that, so far as present data go, normal negative color indices exist in the Hercules cluster in large numbers. From the remarks of the preceding paragraphs it is at once obvious that such a condition could not co-exist with measurable selective absorption of light in space.

The distance of the Hercules cluster, we may be sure, is not less than

1000 units of stellar distance, and it may be nearly ten times this amount. If we were to adopt the value of the absorption constant derived by Kapteyn, then the smallest color indices in the cluster should be in excess of 2.5 magnitudes, and that for a star of spectrum M should be nearly five magnitudes. Actually not a single color index greater than two magnitudes has been observed. If we were to take Van Rhijn's value, the color indices in the cluster would all need to exceed a magnitude.

It seems to be necessary to conclude that the selective extinction of light in space is entirely inappreciable, at least in the direction of the Hercules cluster. If we grant, on the basis of our data, a color excess of a tenth of a magnitude, and attribute it all to space absorption, the value of the coefficient can not then exceed +0.0001 mag., an amount completely negligible in dealing with the ordinary isolated stars. In the light of this result we are probably justified in assuming that the non-selective absorption in space (obstruction) is also negligible.

The following references may be cited:

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STUDIES OF MAGNITUDES IN STAR CLUSTERS, II. ON THE SEQUENCE OF SPECTRAL TYPES IN STELLAR EVOLUTION

By Harlow Shapley

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

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In many investigations of globular clusters the apparent magnitudes of the individual stars, which are the quantities observed directly, may be used without error as representing the absolute magnitudes. We may consider the stars in such a system to be at the same distance from the earth, and knowledge of that distance is unnecessary in discussing the relations between changes in the absolute luminosities of the stars and their colors or spectra.

If, as is most commonly believed, the evolution of the stars progresses, chiefly through the agency of condensation and cooling, from the bluer spectral types to the redder, then, since it is definitely known that the intrinsic surface brightness of the blue stars greatly exceeds that of the red, we should normally expect to find that all the bright stars in the

clusters are relatively blue and that the colors *g* and *k* do not appear until we go several magnitudes fainter than the brightest stars. This condition of increasing redness with decreasing brightness holds with but few exceptions among the stars in the open and moving clusters, such as the systems of the Pleiades, the Hyades, Ursa Major, and Messier 67. In the great globular cluster in Hercules (Messier 13), however, the interval of the four brightest magnitudes contains red and blue stars in approximately equal numbers.

In the following table the average magnitudes in Messier 13 are given for successive concentric regions and for different classes of color. The progression from blue to red is from left to right. As in the preceding paper of this series the data for the stars of the innermost regions of the cluster are not included, though the results would not be affected materially by retaining them. Each magnitude represents the mean value for about 16 stars.

It should be remembered that the system of Messier 13 is immensely remote from the sun and is enormous in its real dimensions, and also that it contains in all probability hundreds of thousands of stars. The longest exposures at Mount Wilson indicate that there are about 50,000 stars in the cluster brighter than magnitude 21, and there is no indication that the fainter limit has been approached. The same conclusions are valid for other condensed globular clusters. Hence, we must recognize that in studying stars between magnitudes 12 and 16 in Messier 13 we are dealing only with those members of the system that have the greatest intrinsic luminosity. Obviously, then, the giant red and yellow stars, which in Russell's hypothesis of the order of stellar evolution play an important part as the antecedents of the bluer stars, are present in great numbers in Messier 13.

TABLE I

AVERAGE MAGNITUDES IN MESSIER 13

LIMITS OF DISTANCE FROM CENTER	COLOR CLASS					ALL COLORS
	b	a	f	g	k	
2′.0 to 2′.5	15.18	15.12	14.88	14.36	12.11	14.76
2 .5 3 .0	15.22	14.75	14.89	14.03	12.74	14.58
3 .0 4 .0	15.16	15.14	14.81	14.18	12.19	14.67
4 .0 5 .0	15.25	15.22	14.92	14.70	13.66	14.91
5 .0 6 .0	15.22	14.96	14.84	14.15		14.62
> 6 .0	15.16	14.03	14.74	14.54	13.69	14.67
Total.....	15.20	15.01	14.85	14.28	12.74	14.71

Although in the relatively near moving clusters, referred to above, there are very few of the giant red stars, their very frequent occurrence among the stars of the general galactic system is commonly recognized. From different lines of evidence we believe them to be tolerably normal stellar objects as regards mass, motions, and surface luminosities, but differing from dwarf red stars most conspicuously in density, dimensions, and total brightness. Admitting this interpretation, there is no place at all for the giant stars in the older and more conventional scheme of stellar evolution, but they harmonize almost completely with the newer evolutionary hypothesis.

From the best data available two years ago, Russell concluded that the giant stars of all spectral types have very similar absolute magnitudes. We are now in a position to make a further contribution to this subject, basing our conclusions upon results from globular clusters. Although later researches may show that our immediate galactic domain is comparable with a globular cluster to a limited extent only, there can be, nevertheless, but little doubt that each globular cluster is in itself a complete system, distinct from the galaxy, and one in which the evolution of the constituent stars probably follows analogous lines to the stellar development in our own system. In many respects it should be possible to read the observed features of the cluster directly into our interpretations of the local system, and the characteristic discussed below is probably such a case.

Table I shows the common dependence of average magnitude upon color class in the different regions of the cluster. The relation is better exhibited in Table II, which gives the average color index for successively fainter intervals of magnitude in different regions. The number of stars included in each average is in parentheses.

TABLE II

AVERAGE COLOR INDICES IN MESSIER 13

MAGNITUDE INTERVAL	DISTANCE FROM CENTER OF CLUSTER			ALL REGIONS
	2'.0 to 3'.0	3'.0 to 5'.0	> 5'.0	
11.8 to 12.6	+1.25 (8)	+1.34 (5)	(0)	+1.28 (13)
12.6 13.2	+0.81 (14)	+0.97 (6)	+0.85 (4)	+0.86 (24)
13.2 13.8	+0.95 (11)	+0.97 (6)	+0.97 (6)	+0.96 (23)
13.8 14.4	+0.71 (27)	+0.72 (36)	+0.81 (13)	+0.73 (76)
14.4 15.0	+0.67 (48)	+0.62 (37)	+0.67 (25)	+0.65 (110)
15.0 15.6	+0.31 (90)	+0.46 (97)	+0.39 (35)	+0.39 (222)

The data of the two tables show conclusively that the brightest stars in the cluster are the red and yellow giants. In fact there are 16 *g*'s

and *k*'s more luminous than the very brightest *b*, which is an exceptional star; and of the 221 brightest stars in these outer regions of the cluster there are only four in color class *b* and twenty in class *a*.

Fainter than the fifteenth magnitude the bluer colors become very common and there is an indication that the reddest stars may be almost entirely absent between magnitudes 15.5 and 17.0. At any rate, between magnitudes 16.0 and 16.8 only four stars out of the hundred measured have color indices greater than half a magnitude, and none is found redder than the sun. If the result is confirmed by later study it will be very important. It would suggest that this magnitude interval represents in the cluster the special domain in the luminosity sequence of the normal first type star, and that the dwarf stars of the redder spectral types (including bodies comparable to the sun) will be found among the still fainter members of the system.

From the foregoing considerations we conclude that not only are giant second type stars present in large numbers in the globular clusters, but also on the average among the four or five brightest magnitudes the brighter the star the redder its color. This condition is the inverse of that for the dwarfs, where, without much doubt, the cooling stars are growing redder, fainter, and smaller with age. In the conventional scheme of the evolutionary transition of spectral types the present result introduces serious difficulties; for Russell's hypothesis, however, the difficulties are not so severe, but still the immediate ancestry of these giant red stars must be taken into account. More detailed considerations of the observations and their interpretation will appear in a forthcoming *Contribution from the Mount Wilson Solar Observatory.*

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EXPERIMENTAL EVIDENCE FOR THE ESSENTIAL IDENTITY OF THE SELECTIVE AND NORMAL PHOTO-ELECTRIC EFFECTS

By R. A. Millikan and W. H. Souder

RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

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In the fall of 1913, while studying the photo-electric properties of freshly cut surfaces of the alkali metals in extreme vacua, we observed that immediately after first cutting, the fresh surface of sodium showed very large photo-sensitiveness when tested with monochromatic light of wave length 5461 A., even when the vacuum was of the order 10^{-6} mm. as measured by a McLeod gauge. But after several weeks of experimenting and many cuttings a condition was reached in which a freshly cut surface was completely insensitive when illuminated with this wave length. The lost sensitiveness reappeared, however, in the course of not more than two minutes after cutting, and grew rapidly to a very large value in fifteen or twenty minutes. When the gas pressure was of the order of 0.01 mm. the same phenomenon occurred but the rise to a maximum value was less rapid. From these results we began to surmise that photo-electric currents must be due to the influence of some active gas, which diffused from the walls to the metal and whose action upon the surface was retarded by the presence of an inert gas.

While we were in the process of testing this hypothesis by repeating the experiment under as diverse conditions as possible the papers of K. Fredenhagen,[1] H. Küster,[2] G. Wiedmann and W. Hallwachs[3] appeared nearly simultaneously, all taking the point of view which had been suggested to us by our own experiments, and the last one of them, namely, that of Wiedmann and Hallwachs, asserting with considerable positiveness that the presence of a gas is a necessary condition for the appearance of photo-electric currents in potassium, and that the "photo-electric effect in general takes place in the mixture formed by the body and the absorbed gas."

By this time, however, we had obtained data which convinced us that the point of view taken by these authors was untenable and in April, 1914, at the meeting of the American Physical Society, we presented a paper entitled 'Effect of Residual Gases on Contact E. M. F.'s and Photo-currents,'[4] in which we described fully and exhibited to the society the curves shown in figure 1. A glance at these curves shows at once that the freshly cut surface is initially strongly photo-sensitive to lines 2535 and 2804 and that its sensitiveness for these lines diminishes

continually with time; while for lines 3126, 3650, and 5461 the surface is initially relatively insensitive and then grows in sensitiveness to a maximum. Now according to Pohl and Pringsheim,[5] the selective effect in sodium shows a resonance band whose maximum is at about 340 $\mu\mu$. It is obvious then that if we wish to define the selective effect as the strong photo-sensitiveness in the neighborhood of this wave length, then these curves mean that the selective effect so defined is due to the presence of gas and grows as the gas has time to get at the surface. Since we did not wish to define the selective effect in any such arbitrary manner we interpreted our result in the following words:

A freshly cut sodium surface is not sufficiently electro-positive to respond photo-electrically to light of wave-length 5461 A., but under the influence of an

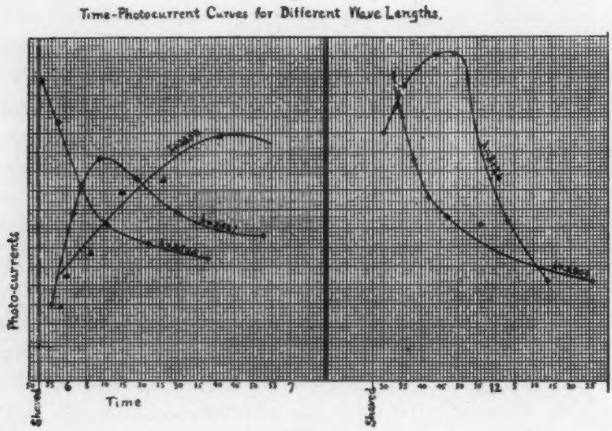

FIG. 1

active gas, the sodium forms a new substance which is more electro-positive than the freshly cut sodium and hence is photo-sensitive to longer waves. The photo curve taken with wave-length 5461 A., represents then merely the growth and decay of this substance. For sufficiently short waves, however, the freshly cut surface is itself so photo-electrically active that its own decay curve completely masks the rise and fall curve due to the growth and decay of the more electro-positive substance resulting from the action of gas upon the sodium.

In view of these results the authors raise a question as to the correctness of the conclusions of Wiedmann and Hallwachs that photo currents are obtainable only in the presence of gas. We suggest that if the Wiedmann and Hallwachs experiments are repeated with light of sufficiently short wave-lengths instead of with visible light, it is not likely that the photo-sensitiveness will

be found to vanish in the way in which it did in the experiments reported by these authors.

This article formed the subject of some correspondence between Professor Hallwachs and one of us, and quite recently Wiedmann[6] working in Hallwachs' laboratory has followed out the suggestion above made and obtained by cleansing his surfaces by successive distillation instead of by cutting, as we had done previously, results altogether similar to ours, though of less pronounced character and extending over a much smaller range of wave-lengths. Wiedmann's actual observation is that potassium, whose selective resonance band, according to Pohl and Pringsheim, is at 436 $\mu\mu$, shows a relatively stronger photo-sensitiveness for line 365 $\mu\mu$ than for either 406 $\mu\mu$ or 436 $\mu\mu$, when it has been freed from gas by repeated distillation. But when distillations are less numerous and freedom from gas less complete the photo-sensitiveness of this line is some four times greater than that of the other lines. This means merely in terms of our diagrams that lines like 2535 A. and 2804 A. which have wave lengths shorter than that corresponding to the selective point are relatively strong with respect to those whose wave-lengths are at or above that of the selective point when gas has had little chance to act, but that they become relatively weak after the gas had had a chance to act.

These results from Professor Hallwachs' laboratory constitute then very interesting confirmation of both the results and the conclusions which we published one and one-half years ago, namely, that when tested with sufficiently short wave-lengths, freeing a surface from gas does not at all eliminate its photo-electric sensibility, though, as we pointed out then, it does diminish the photo-sensitiveness on the long wave-length side, i. e., at or above the region of the wave-lengths at which Pohl and Pringsheim's selective effect reaches its maximum.

Within the past year we have made experiments which show that it is very difficult if not impossible to set up any definite criterion for the selective effect since the so-called normal and selective effects merge by insensible gradations into one another. At present we prefer to regard the selective effect as nothing but the normal effect taking place in the neighborhood of an absorption band, i. e., in the neighborhood at which oscillations of one particular frequency predominate. Our reasons for this point of view are as follows:

(1) We have shown that the energy of emission is always governed by the normal equation, $\frac{1}{2}MV^2 = PDe = h\nu - P$, whether the impressed frequency coincides with the maximum of the selective effect or not.

(2) We have found that the angle of incidence or azimuth of polarization is not at all essential to the appearance of a strong maximum at the wave length at which Pohl and Pringsheim locate the frequency of the maximum selective effect, for we find this strong maximum when we work not only with unpolarized light but with normal incidence. In other words, the only condition for the copious emission of corpuscles is the coincidence of an impressed frequency with a natural frequency.

(3) We have found that with a clean surface the curve expressing the relation between the frequency and the photo-current per unit of incident energy rises steadily with increasing frequency right through the point assigned by Pohl and Pringsheim for the maximum of the selective

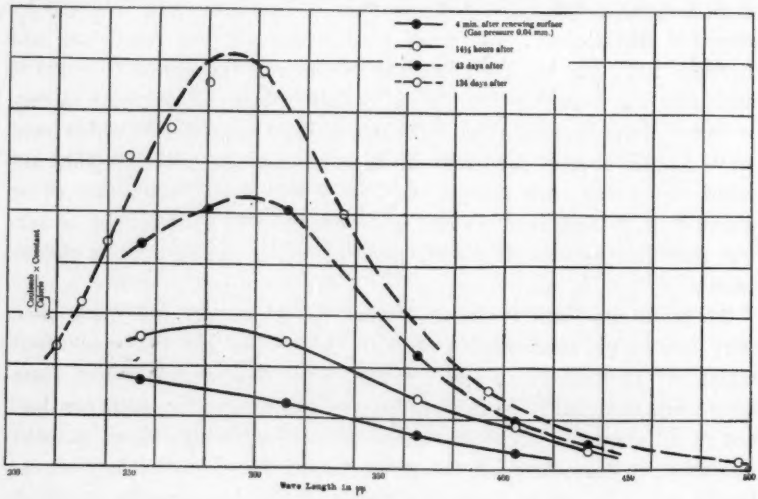

FIG. 2

effect and that as a surface has opportunity to be acted upon by gas a maximum slowly grows at the frequency indicated, and ultimately becomes very large. This means that if we regard the maximum as a criterion of the selective effect, then the selective effect merges by insensible gradations into the normal so that no sharp line of demarcation can be set up between them. The results stated under (1) follow from the photo-electric determination of Planck's '*h*.'[7] Those stated under (2) and (3) follow from the data plotted in figure 2. This data was taken as follows:

Light from a 220 volt quartz-mercury lamp was passed through a Hilger monochromatic illuminator into a vacuum chamber in which the

alkali metals, lithium, potassium and sodium, were arranged on the periphery of a wheel which could be rotated electro-magnetically by an electromagnet outside the chamber. Fresh surfaces of these metals were produced by means of a knife operated from outside by another electro-magnet. A Hilger thermopile used in connection with a Coblentz galvanometer sensitive to 1×10^{-11} amperes intercepted a portion of the beam and permitted energy measurements to be made simultaneously with the measurements of the photo currents.

The apparatus was the same as that used in the photo-electric determinations of Planck's 'h' with the aid of the alkali metals.[7] As indicated above, the incidence was always normal.

The photo-currents are all saturation currents with -12 volts applied to the lithium.

The pressures at which the observation in the experiments corresponding to curves in figure 2 were made ranged from 0.04 to 0.0001 mm.

While the rate at which the maximum grows depends somewhat upon the pressure, any individual curve is found at a given time to be not essentially modified, so far as the phenomenon here under consideration is concerned, by changes in pressure within the specified limits.

Changes in photo-sensitiveness bearing some similarity to these were observed by Pohl and Pringsheim[8] in the cases of barium, aluminium, magnesium and calcium but the maxima observed by them were not shown to be in the neighborhood of a wave length at which a selective effect had been found. Indeed Pohl and Pringsheim did not interpret their results as meaning that these four metals possess any selective effect at all. It will be observed, however, that the maximum in curve 2 coincides, within the limits of accuracy of Pohl and Pringsheim's determination, with the frequency 280 $\mu\mu$ at which they located the selective effect of lithium. It is not at all likely, therefore, that our normal effect can differ in any essential particular from their selective effect in lithium. It is true that we have not shown that our lithium surface shows the characteristic of yielding an energy-frequency curve having a maximum when light is polarized perpendicular to the plane of incidence combined with no maximum when it is polarized parallel to the plane of incidence. But this was not the criterion which Pohl and Pringsheim[9] used when they discovered the existence of the selective effect in lithium. Their criterion was simply the existence of a pronounced maximum at 280 $\mu\mu$ when they used oblique incidence and polarized light. It is scarcely conceivable that our maximum at 280 $\mu\mu$ would disappear if we made our tests under precisely the same conditions which they used.

Our experiments have, as yet, thrown no light on the cause of the ex-

ceptionally profuse emission shown by potassium and sodium when the electric vector of the light wave vibrates normally to the surface. But so far as we can discover the selective effect in lithium has never been shown to possess this characteristic.

Summary.—1. Photo-electric phenomena do not appear to be in general conditioned by the presence of a gas, since they appear with the use of sufficiently short wave lengths even when they do not appear with the use of long wave lengths.

2. The long wave-length limit of photo-sensitiveness is modified by the presence of a gas and this long wave-length limit is determined by the presence of a substance which gives rise to a maximum photo-sensitiveness at the so-called resonance wave-length.

3. Some of the characteristics of the selective effect have been obtained under the conditions under which only the normal effect is commonly observed.

4. The curves commonly supposed to be characteristic of the normal effect may be made to merge by insensible gradations into those of the selective effect by the action of gas on the surface of the metal.

5. All distinctions between the normal and selective effects in lithium have disappeared.

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CONCOMITANT CHANGES IN TERRESTRIAL MAGNETISM AND SOLAR RADIATION

By L. A. Bauer

DEPARTMENT OF TERRESTRIAL MAGNETISM, CARNEGIE INSTITUTION OF WASHINGTON

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While good progress has been made by various investigators in establishing the relationship between fluctuations of the earth's magnetism and those of the sun's activity during the sun-spot cycle, there are still outstanding a number of important questions. The magnetic quantity hitherto generally used, as for example, one of the magnetic elements (chiefly the magnetic declination), or the range of the diurnal variation

(again chiefly of the magnetic declination), has not admitted always of direct physical interpretation, nor has it furnished always a convenient measure of the magnetic changes. Accordingly, the author in a preliminary examination of this relationship, made in 1909, introduced a quantity, called the 'local magnetic constant,' designated by G, which, under certain assumptions, is proportional to the magnetic moment of the earth, or to the intensity of magnetization.

Various recent investigations have shown that the quantity, G, provides an adequate measure of certain changes to which the earth's magnetism is continually subject. One interesting result of the 1909 investigation was, that increased solar activity, as measured by sun-spot frequencies, was accompanied apparently, by a decrease in the earth's magnetic constant. This is the general effect which accompanies any large magnetic disturbance. For example, during the magnetic storm of September 25, 1909, the earth's magnetic state was below normal for a period of about three months. Since magnetic storms in general increase in frequency, as well as in magnitude, with increased sun-spot activity, the general effect on the magnetic constant during the sun-spot cycle is as it was found to be.

In the present paper there are considered changes in the earth's magnetism of a considerably minor order of magnitude as compared with the magnetic perturbations just discussed; however, they are found to be not less important. The precise relationship between changes in solar radiation and possible changes in the earth's magnetism could be subjected to a definite examination only when values of the solar constant, of such accuracy as those of the Smithsonian Institution, became available. Fortunately, we now have a series of determinations at Mt. Wilson of this constant by Abbot for a period of four to five months during the years 1905 to 1914, excepting 1907. The 1913 and 1914 data were kindly supplied by him, in advance of publication, for special use in connection with the present investigation. There were likewise made available the magnetic data for the same years, recorded at the observatories of the Coast and Geodetic Survey, for which acknowledgment should be made to the Superintendent of that Bureau.

In the Balfour-Schuster theory of the diurnal variation of the earth's magnetism, it was necessary to introduce an additional hypothesis to account for the great ionization required by the theory, and solar radiation suggested itself as a possible cause. "Hence," Schuster says, "we might expect an increased conducting power in summer and in daytime as compared with that found during winter and at night." If solar

radiation plays the prominent part required in the Schuster analysis of the diurnal variation of the earth's magnetism, the question naturally arises: If, at any particular moment or period, the solar radiation falling upon the earth's atmosphere, suffers, from some cause, an appreciable increase or decrease, is there a corresponding observable magnetic change? A diminution, for example, in the amount of solar radiation, could be caused by the interposition of some screening body between the sun and the earth. The interposing body might be the moon, as during a total solar eclipse, or a cooling layer above the sun's photosphere. In the first case, magnetic observations made during a total solar eclipse would shed some light, and in the second case a comparison of observed values of the solar constant with concomitant magnetic records would be of great interest. We have carried out both lines of inquiry.

It is not possible to enter here into the details of all tests applied and as to methods of computation employed. It must suffice to state the chief conclusions derived to date.

a. Changes in the earth's magnetism of appreciable amount are found associated with the changes in solar radiation as shown by values of the solar constant possessing the requisite accuracy. For the average daily change in the solar constant, which amounts to about 1.5% of its value, the magnetic constant, used as a measure of the prevailing magnetic state of the earth, suffers a change of about 0.003%, or about one digit in the fifth decimal C. G. S. units. The effect on the horizontal component of the earth's magnetic force would be about twice this.

b. Decreased solar constant appears to be accompanied by increased magnetic constant and decreased diurnal range of the earth's magnetism, in accordance with the following relations: 1% change in the solar constant is accompanied by a change of about 0.002% in the magnetic constant, and by about 1% in the magnetic diurnal range. Assuming for the present a linear relation between the solar-constant and magnetic changes, a 10% change in the solar constant, as occasionally occurs, may be accompanied by a change in the magnetic constant of about 0.02%, and by about 10% in the magnetic diurnal range. The magnetic effects observed during total solar eclipses are about equivalent to those which might be expected from about a 10% change in the intensity of solar radiation.

c. Since the changes in solar radiation are aperiodic and occur more or less spontaneously, the effect on the earth's magnetism is generally of a threefold character: (1) An alteration in the diurnal range, (2) perturbations both of the world-wide and the local kinds, (3) an out-

standing residual effect such as to alter the daily mean values of the magnetic elements by an amount 10 to 100 times that caused by the regularly-progressing secular variation. The magnitude of the effects may at times exceed the average ones described in (*a*) and (*b*), dependent upon peculiar local conditions (ionizations) of the upper atmospheric layers. Changes in solar radiation may thus furnish sufficient cause for the ever-present minor perturbations and elementary waves, or pulsations, of the earth's magnetism.

d. The daily non-cyclic changes in the earth's magnetism, as found on magnetically-quiet days by previous investigators, furnish an additional check on the foregoing results, their quantities harmonizing completely, both as regards sign and magnitude with those given here. It is found that on consecutive quiet days the magnetic constant is, on the average, larger on the second day than on the first, the increase being equal to that which would be caused by an average daily change in the solar constant. Moreover, the reason why the magnetic constant, or the horizontal intensity, is larger, on the average, on the second quiet day, is because, on the average, the solar constant is slightly smaller on the second day than on the first. The relation between solar change and magnetic change during consecutive quiet days is precisely of the same sign and amount as given in (*b*).

e. If the quiet day magnetic effect were to persist throughout the year, it would cause a secular variation fully ten times that generally observed. However, the quiet days are in the minority, being exceeded three times and more by unquiet days, on which the magnetic effect is of an opposite or compensating kind to that of the quiet day. Since these acyclic effects appear to be associated with solar changes and since the latter are not periodic, but more or less sporadic, there is an outstanding effect at the end of the year which causes an irregularity in the regularly-progressing secular change. Accordingly, there should be found some correspondence between annual changes of the solar constant and annual magnetic changes. This is found to be the case. Since the solar-constant changes occur only approximately in accordance with sun-spot activity and since the magnetic changes are found to conform so closely to those in the solar constant, an explanation is obtained as to why the irregularities in the magnetic secular change do not always synchronize with changes in solar activity as measured by the sun-spot numbers nor correspond in magnitude to them.

SUB-MARINE SOLUTION OF LIMESTONE IN RELATION TO THE MURRAY-AGASSIZ THEORY OF CORAL ATOLLS

By Alfred Goldsborough Mayer

DEPARTMENT OF MARINE BIOLOGY, CARNEGIE INSTITUTION OF WASHINGTON

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In 1880 Murray[1] advanced the idea that the solvent action of sea-water for limestone was a primary factor in the deepening and widening of the lagoons of coral atolls. This idea was afterwards adopted by Alexander Agassiz[2] who became its most active advocate. In 1914 Dole[3] made a chemical study of the water of the lagoon of the atoll of Tortugas, Florida, and decided that it contained no free carbon dioxide, and Tashiro[4] pursuing a different method decided that if free carbon dioxide be present in Tortugas sea-water its amount must be slight. In view of these facts Vaughan[5] stated that lagoons of atolls could not have been dissolved out by sea-water as such, but he gave no quantitative data to support this view. The validity of the Murray-Agassiz theory depends upon the *rate* of the solution of limestone in atoll lagoons. Elschner[6] states that the solubility of calcium carbonate in sea-water is extraordinarily low but he also gives no quantitative data. I have made an attempt to determine the rate of solution of calcium carbonate in Tortugas sea-water. Carefully weighed pieces of the shell of the mollusc *Cassis* having a specific gravity of 2.88 and areas ranging from 57.6 to 85 sq. cm. were treated as follows:

Shell No. 1 was placed for 367 days in a sealed, sterilized, glass carboy containing 45 liters of doubly filtered sea-water, which after being filtered was heated within the carboy to 72.5°C. for two hours. At the end of the year this sea-water still retained 71% of its alkalinity to phenolphthalein test. No bacteria or other visible growths developed within this carboy and the shell remained clean and without slime upon its surface. This shell weighed 13.285 grams in July, 1914 and at the end of the year it had lost 0.014 gram. Thus it would have taken 948 years to dissolve the shell, and a thickness of 0.00067 mm. was removed from its surface by one year's immersion.

Shell No. 2 was placed in another similar carboy, but the sea-water was neither filtered nor sterilized, and it developed bacteria, protozoa and algæ, and at the end of the year it was acid; its free acidity over and above neutrality by phenolphthalein test being equivalent to $\frac{n}{3000}$ H_2SO_4. The shell originally weighed 15.532 grams and it lost 0.173 gram after 360 days. Hence it would have taken 90 years to dissolve the shell, and a superficial thickness of 0.007 mm. was removed in one year.

Shell No. 3 was placed for 364 days in a 15 liter glass bottle which was enclosed in a wooden dark chamber to prevent the growth of plants. The water was mechanically changed by each rise and fall of the tide, flowing in and out of small glass tubes, thus preventing the formation of strong currents within the bottle. This bottle was placed in the marine moat surrounding Fort Jefferson, Tortugas. This moat-water supports an abundant animal life and contains more CO_2 than the open ocean, but remains always alkaline to phenolphthalein. At the beginning of the experiment the shell weighed 10.5435 grams, and at the end of the year it had lost 0.0115 gram. Thus it would have taken 917 years to dissolve the entire shell and a superficial thickness of 0.00069 mm. was removed in one year.

Shell No. 4 was placed for 364 days in a similar glass bottle which remained between tides off the western wharf of Loggerhead Key, Tortugas, thus being surrounded by open sea-water. Considerable silt was drawn in through the glass tubes, and the shell was found buried beneath about 8 mm. of limestone mud which was charged with H_2S. A number of tunicates, shrimps, molluscs and worms were living within the bottle at the end of the year, but the circulation had become interrupted by the growth, within the bottle, of a tunicate across the opening of one of the tubes. The shell originally weighed 15.22 grams and it had lost 0.047 gram, amounting to a superficial thickness of 0.0019 mm. in a year. Thus it would have required 324 years to dissolve the shell.

Taking the results of this last experiment as a maximum rate of solution of calcium carbonate by sea-water as such, it appears that 19,250,000 years would be required to dissolve out a layer of calcium carbonate to a depth of 20 fathoms, this being about the average depth of atoll lagoons; and as many if not all atoll lagoons have been formed since the beginning of Tertiary times it appears that they owe their development to agencies other than that of solution by sea-water, for even if the rate of solution of reef limestone is 100 times as rapid as that of the *Cassis* shell it would require 192,500 years to dissolve out an average lagoon.

Holothurians, Echini, boring algæ, certain sponges, worms, dead organisms, and probably most important rain water washing out from forested shores are agencies which dissolve limestone but their effects have not yet been quantitatively evaluated. Thus the lagoons of barrier reefs surrounding volcanic islands may have been formed in some appreciable measure by solution, but this cannot apply in the same degree to the lagoons of atolls where the land area is insignificant in

proportion to the area of the basin of the lagoon, and we know from the studies of Guppy, Wood-Jones, and Vaughan that many lagoons are filling up with sediment. In fact calcium carbonate is being precipitated from the sea-water of the Florida-West Indian region in the manner determined by Drew[7] and Kellerman and Smith;[8] the precipitate finally changing into oolite as observed by Vaughan.[9]

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[5] Vaughan, T. Wayland, Publication No. 182, The Carnegie Institution of Washington, p. 62, (1914); also 1914; *Bull. Geol. Soc. Amer.*, **26**, 58 (1914).

[6] Elschner, C., *The Leeward Islands of the Hawaiian Group, Honolulu*, p. 48, (1915).

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[8] Kellerman, K. F., and Smith, N. R., *Washington, J. Acad. Sci.*, **4**, 400 (1914).

[9] Vaughan, T. Wayland, Carnegie Institution of Washington, Publication No. 182, p. 47–54 (1914).

THE ARCHEGONIUM AND SPOROPHYTE OF *TREUBIA INSIGNIS* GOEBEL

By Douglas Houghton Campbell

DEPARTMENT OF BOTANY, STANFORD UNIVERSITY

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Treubia insignis is a remarkable liverwort discovered by Goebel in Western Java. It is one of the largest and most striking species known and is of special interest as being in some respects the nearest to the typical leafy liverworts of any of the 'anacrogynous' Jungermanniales.

For some time after its discovery Treubia was known only from the original locality near Tjibodas on Mt. Gedeh in Western Java. It has since been discovered in several widely separated regions, e.g., New Zealand, Tasmania, Patagonia, Tahiti, and Samoa. In May 1913, I collected a single specimen on Mt. Banajao in Luzon, Philippine Islands.

The present paper is based upon material collected at the original station (Tjibodas), in 1906. The material comprised female and sterile plants with gemmae, but no male plants were found.

The archegonia are in groups, up to a dozen, protected by the characteristic dorsal scales which occur at the base of each leaf.

The development of the archegonium is much like that of other liverworts, but shows one striking difference—instead of the usual five or six rows of peripheral cells in the neck of the archegonium, there may be as

many as nine, and there is not a clear line of demarcation between the neck and the ventral part of the archegonium. None of the archegonia examined by me showed more than eight neck canal cells, but in a recent paper on Treubia by Grün, it is stated that in the mature archegonium there are sixteen.

The earliest stages of the embryo were not seen. In the youngest that were found, the basal portion of the embryo was occupied by a large haustorium formed of large cells very similar to that observed in several other Anacrogynae; e.g., Pallavicinia, Podomitrium, Aneura, but not found in Fossombronia, with which Treubia has a good many points in common. The haustorium is probably formed by the first division in the embryo, and this division, as in all other Jungermanniales, is presumably transverse. The portion of the embryo above the haustorium becomes later differentiated into foot, seta, and capsule, and the young sporophyte closely resembles corresponding stages in Podomitrium or Pallavicinia. The foot is not clearly delimited from the seta.

In the upper region (capsule), the differentiation of the sporogenous tissue takes place at a rather late stage and the limits of the sporogenous tissue are much less definite than is the case, for example, in Aneura, Fossombronia or Pallavicinia. Outside the sporogenous area there are about three layers of sterile cells constituting the wall of the capsule. At the apex the number of cell layers is greater—as many as eight may be present—and a conspicuous beak is formed as in Podomitrium and Pallavicinia.

There is no elaterophore and no evident relation between the spore mother-cells and the elaters. The latter are few in number, but reach finally an extraordinary length—1.25 mm. according to Grün. . There is no perianth or involucre about the archegonia, but a very massive calyptra is formed about the young sporophyte. The calyptra may finally reach a length of 1.5 cm. The ripe capsule is ovoid in outline, not globular as Andreas asserts. It opens by four broad and somewhat irregular valves.

Treubia is probably on the whole nearer the leafy liverworts (Jungermanniales Acrogynae) than is any other anacrogynous genus. Among the latter Noteroclada probably is most nearly related to Treubia, and connects it with Petalophyllum, which in turn is not very distantly related to Fossombronia. The latter shows some significant resemblances to the lowest of the liverworts—the Sphaerocarpales, and it may be necessary to separate as a special order or sub-order the series beginning with Sphaerocarpus and ending in Treubia, which in turn is almost certainly connected pretty closely with the acrogynous Jungermanniales.

BRIEF NOTES ON RECENT ANTHROPOLOGICAL EXPLORATIONS UNDER THE AUSPICES OF THE SMITHSONIAN INSTITUTION AND THE U. S. NATIONAL MUSEUM

By Aleš Hrdlička

DIVISION OF PHYSICAL ANTHROPOLOGY, U. S. NATIONAL MUSEUM

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In the April and July (1915) numbers of these PROCEEDINGS I have given notes on 'Some Recent Anthropological Explorations' carried out under the auspices of the Smithsonian Institution, and on 'An Exhibit in Physical Anthropology' which was prepared on the basis of these explorations for the Panama-California Exposition; I will now complete the account by referring in brief to the scientific results of the several expeditions made in this connection.

The field work was directed towards three main objects, namely, the securing for this country of original specimens relating to earlier man in Europe and Asia and contributing thus to the advance of knowledge in this direction; the initiation of comparative study of the child among primitive peoples; and the search in Asia for possible traces of the ancient stock of humanity which gave us the Indian; while an additional aim was to complete as far as possible our collections of skeletal material bearing on prehistoric American pathology. Some of the results of these activities have already been published, at least in a preliminary form (see bibliography, April number), and need not be referred to again at this time; while the remainder can be summarized as follows:

Search for Neolithic Human Remains in Southwestern Russia.—It is well known[1] that southwestern Russia and particularly the province of Ukraina, is rich in mounds or 'kurgans,' which yield human remains dating from the early historic back into the neolithic period. It is the region which in the past has yielded bones colored red,[2] and also some crania of most interesting form, partly transitional with those of geological antiquity. The exploration was entrusted to Prof. Kazimir Stolyhwo, Chief of the Anthropological Laboratory at Warsaw, and was restricted to the district of Kiev, in the vicinity of the villages of Szulaki, Puhaczówka, Chejtowa, Zywotówka, Tackowica, Zacisze and Horodnica.

The total number of Kurgans explored was thirty-three, twenty-seven of which yielded human remains, which, however, in the majority of cases had been disturbed. The mounds averaged close to 90 feet in diameter, the range being from approximately 40 to nearly 300 feet; and nearly 5 feet in height, or from less than a foot to nearly 8 feet. Most if not all of the tumuli were originally higher, being reduced in the

course of time by agriculture. The tops of several of the mounds were covered with stone, which must have been brought from a distance. The number of human burials encountered in these mounds was upwards of 70. They were located generally about or not far from the center of the mound and at varying depths, from a few inches to nearly 15 feet beneath the summit. Some of the burials were plainly intrusive. The old burials included individuals of both sexes and all ages. The bodies were as a rule in moderately contracted position. In the majority of cases the bones were colored more or less red, due to the inclusion in the graves of red ochre. The mode of burial differed. In one mound it was by incineration of several individuals; in others a fossa had been made in the surface of the ground, in which the body or bodies were placed, the grave being covered by a low shelter of wood, about and over which was piled the soil from the immediate neighborhood. In still other instances the body was simply buried in the earth. Most of the mounds contained also traces of ceremonial fire and of animal bones, some of accidental inclusion, but some probably from offerings. The archeological remains were scarce, though many may have been removed by treasure hunters previously. With two skeletons, in two separate but adjacent mounds, there was found iron; with three or possibly four, were traces (stains) of brass or copper; with one a small ornament of gold; with six there were old articles of bronze such as ear pendants and bracelets; with six there were objects of bone, such as heads, or artificially perforated teeth of carnivores; with two burials there were implements of stone, and fourteen of the mounds yielded primitive hand-made pottery. The older skeletal remains of man are of special interest. Although in poor condition, they show a uniform dolichocephalic type of people, of good stature; there is no evidence of any superposition of types or even mixture until we come to relatively recent burials. The animal bones recovered from the various mounds comprise those of three or four species now extinct in these regions, namely: *Bison bonasus*, *Equus* (prob. *gmelini*), *Ochotona pusilla*, and *Marmota bobak;* and those of some of the common living ungulates and carnivora, a few birds, with a variety of rodents. Detailed report on these finds is in preparation by Professor Stolyhwo.

Explorations in the Birusa caves and rock shelters on the Yenisei River, Siberia.—During my trip along the Yenisei River in 1912, my attention was forcibly attracted by a large number of caves showing from a distance in the cliffs of a wild region about the mouth of a small stream known as the Birusa, on the left bank of the river about 50 miles south of Krasnoyarsk. The whole locality is known as Birusa and, as I

learned later, some of the caves and nearby tumuli have been previously explored in part by M. Jelenief of Krasnoyarsk, who discovered in them remains pointing in particular to the Neolithic period. As the whole region west of the Yenisei in this latitude is very rich in mounds and other remains of old populations, some of which are known to date back to the Neolithic period, it seemed most desirable to subject the Birusa caves, as far as possible, to scientific exploration, and the work was intrusted to Professor Stolyhwo, who the year before explored the mounds in Ukraina. The work was carried out during the latter part of the summer of 1913, but was greatly interfered with by adverse weather and other unfavorable conditions. Nevertheless, excavations were completed in three of the caves, four rock shelters and one mound. The results were more encouraging than conclusive. With the exception of few teeth, no skeletal remains of man were discovered; but the caves and shelters yielded numerous traces and examples of bone and stone industry. A detailed account of this exploration also is being prepared for publication by Professor Stolyhwo. Careful and extended exploration of the region is most desirable. The many remaining caves about the Birusa and the thousands of mounds over the steppes to the westward, only a few of which have as yet been touched, invite urgently the hand of the archeologist.

STUDIES ON THE DEVELOPMENT OF THE CHILD AMONG PRIMITIVE PEOPLES

The Negrito.—In 1912, after receiving due instructions, Dr. Philip Newton of Washington, D. C., was sent to the Philippine Islands to study the child among the Negrito. His stay in the Philippine Islands extended over 103 days, of which however only 65 could be spent in actual field work, the remainder being taken up by travel, storms and by illness incurred in the unhealthy environments. During the trip more than 1000 Negritos were seen, of whom 400 were measured and observed. These included 348 full bloods and a little over 50 mixed bloods. The mixed bloods represented mostly the Bataks of the Island of Palawan; the full bloods were in the main from Luzon. Among the pure bloods examined, there were 75 children. The rather small proportion of children to adults was due to the fact that many adults came for the examination for some distance, without bringing with them their families, and partly to the fact that the number of children per family among the wild Negrito is exceptionally small.

Photography and particularly cast making met with serious difficulties, due to the tropical climate and great moisture. About 300 photographs were nevertheless taken, 70 of which, however, were ruined when

the boat carrying the expedition was upset during a severe storm on the Cagayan river. As to cast making, although plaster was taken along packed in supposedly air tight cans, when these were opened it was found to have absorbed so much moisture that it was quite useless; and an additional supply purchased in Manila yielded no better results. Collection of skeletal material also met with considerable difficulties. The people, and hence their burials, are widely separated, and when the latter were found the bones, in a large majority of cases, due to the excessive moisture, were in such poor condition that they could not be collected; twelve graves yielded few parts only of a single skeleton.

A great obstacle encountered in the examination of the Negrito children was the absence of reliable data as to their ages. Nevertheless, the series is an interesting and valuable one and can be confidently expected to throw light on a number of important features of the child's development among these exceedingly primitive people, who besides live in a highly specialized environment. The data are being prepared for publication.

African Negro.—Perhaps the most important large ethnic group for anthropological studies, particularly those on the child, is that of the pure African Negro, and an expedition of over 14 months' duration was carried out under my direction, among several of the more favorably situated tribes of this race, by Dr. A. Schück, of Prague. Principal attention was given to the Zulu, from South Natal to North Zululand, and the results are measurements of upwards of 1000 children and adolescents of this people. These children were reached through the various Mission schools and in nearly all cases the age could be exactly determined. The results of the measurements and observations, which are already in an advanced stage of preparation for publication, are expected to afford a solid basis of data concerning the child development of the pure blood negro of good stock, which in the future should be of considerable value in connection with studies on the development of our own colored population.

Besides the Zulu, the expedition reached a limited number of the Bushmen and was engaged on the tribes in British East Africa, including some pygmies, when the outbreak of the war in Europe brought the work forcibly to an end.

Besides measurements and observations, the results of this expedition consist of numerous photographs; a series of excellent facial casts of the Zulu, and an especially valuable series of 20 facial casts of the Bushmen. There were also made on this trip numerous collateral observations of physiological and medical nature.

The Eskimo.—About the most exceptional environment under which man lives is that of the Eskimo, and it would seem that if anywhere, it would be among these people that the effects of environment on development should be encountered. This decided me to send an expedition to one of the largest and for purposes of research the most favorably situated groups of these people, namely that of St. Lawrence Island. This island lies, as is well known, in Bering Sea, approximately 40 miles from the Siberian and a little over 100 miles from the nearest point on the Alaskan coast. The work was entrusted to Mr. Riley D. Moore, D.O., aid in my laboratory in the National Museum, and after due preparation he left for the Island in the spring of 1912, returning late in the autumn. He found the Island inhabited by 292 full-blood Eskimo, of which he measured and examined 180, including a majority of the children; with young infants the mothers objected to all handling. Fortunately, as among the Zulu, the ages of the children could in a large majority of cases be exactly determined, the Island having for years had the advantage of a Government school; in consequence the data secured can not fail to be of considerable scientific value, and are being gradually prepared for publication. Besides the measurements and anthropological observations, the expedition secured a good series of photographs of persons of all ages, a series of facial casts, a collection of skeletal material, and much general information.

Native Siberians.—In addition to the above researches, it was planned to examine into the development of the Australian child and the Chinese. Unfortunately neither of these plans could be realized, but instead it became possible to send a well qualified observer, Dr. S. Poniatowski, chief of the Ethnological Laboratory in Warsaw, to some of the tribes in the Primorskaia province of East Siberia. Unfortunately this expedition, undertaken in 1914, was broken up by the war; yet measurements were secured on 109 Golds and 25 Orochons, including a good number of children. Numerous photographs were also taken, but only one facial cast could be made on account of prevailing apprehensions regarding the purpose of the work. It was hoped that this expedition could be resumed during the fall and winter of 1915–1916, but present conditions make this impossible.

On the whole, then, we have succeeded in securing an excellent series of data on the children of the African negro; a valuable though not large series of similar data on the Eskimo child; a series of some value on the child of the Negrito; and some data among the native tribes of eastern Asia. I may add that we have already a good series of similar observations, made by the same methods, on the Indian child.[3] It may there-

fore be stated that, notwithstanding the many difficulties in the way of such studies among primitive peoples, there have been made some serious beginnings in this very important line of anthropological investigation. As to detailed results, nothing can be said until a careful and necessarily tedious elaboration of the data is completed.

The third main object of anthropological field work under the auspices of the Smithsonian Institution during the last three years was the search in Asia for *probable traces of the ancient stock or stocks of mankind, from which the American Indians were derived.* On this subject a number of preliminary reports have already been published[4] and it is unnecessary to do more in this place than to state that such traces undoubtedly exist in Asia to this day, that they extend over a very large territory, and that they are soon to receive further attention by the Smithsonian Institution.

[1] See bibliography in A. A. Ivanovskiji, Ob Antropologičeskom Sostavě Naselenii Rossii, Moskva, 1904.

[2] See Hrdlička's The Painting of Human Bones, etc. *Smithsonian Inst. Rep.* for 1904.

[3] Published in *Smithsonian Inst., Bull. Bur. Amer. Eth.*, No. 34.

[4] Symposium on The Problems of the Unity or Plurality and the Probable Place of Origin of the American Aborigines, *Amer. Anthrop.*, **14**, No. 1, Jan.–March, 1912; Restes, dans l'Asie orientale, de la race qui a peuplé l'Amérique, *Congrès International d'Anthropologie et d'Archéologie préhistoriques, Compte Rendu de la XIV Session*, Genève, 1912; Remains in Eastern Asia of the Race that Peopled America, *Smithsonian Inst., Misc. Coll.*, **60**, No. 16; The Derivation and Probable Place of Origin of the North American Indian, *Proc. XVIII International Congress of Americanists*, and the Genesis of the American Aborigines, *Proc. II Pan-American Sci. Congress*, Washington, 1916.

A THEORY OF NERVE-CONDUCTION

By Alfred Goldsborough Mayer

DEPARTMENT OF MARINE BIOLOGY, CARNEGIE INSTITUTION OF WASHINGTON

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This research is a continuation of the studies reported in these PROCEEDINGS, **1**, 270, May 1915, and it will therefore be unnecessary to redescribe methods of experimentation. These later experiments were made at the Marine Laboratory of the Carnegie Institution of Washington at Tortugas, Florida, in July, 1915, and the kymograph records were studied in detail in a room which the writer was privileged to occupy in Guyot Hall, Princeton University.

In the experiments of 1914 the distilled water contained between 10^{-5} and 10^{-6} H ion concentration due to carbon dioxide, thus giving an excess of free H ion. The endeavor was therefore made in July, 1915, to obtain neutral distilled water in which both the H and the OH ion concentration approached 10^{-7}. Accordingly, following the suggestion of

Prof. G. A. Hulett, the distilled water was freed in so far as possible from CO_2 by bubbling through it for 60 hours air which had passed through glass tubes containing granulated soda-lime. As the water still appeared slightly acid to phenolpthalein it was rendered alkaline by adding NaOH until the concentration of the OH ion was about $0.75\,n. \times 10^{-5}$.

Although this water was kept in a glass carboy carefully protected from contact with air containing CO_2 it nevertheless became slowly less and less alkaline, probably due to decomposition of traces of organic matter, so that in the course of 11 days the distilled water became nearly neutral, both the H and OH ion concentration approaching 10^{-7}.

The use of this nearly neutral distilled water in diluting the sea-water should enable one to determine the true effects of the sodium, calcium, and potassium cations of the sea-water without serious disturbances due to H or to OH ions, both of which in weak concentration accelerate and in strong concentration depress the rate of nerve conduction.

Thus we find that a concentration of about 6×10^{-5} OH in sea-water is a maximal stimulant for nerve conduction, but becomes a depressant if its concentration be still further increased.

Experiments were made upon 71 *Cassiopea xamachana* medusæ between July 5 and 16, 1915; the OH ion concentration in the distilled water declining during the course of the experiments from about 10^{-5} to about 10^{-7}. Thus for the first 25 medusæ, when the OH ion concentration was at the start about 10^{-5} and was approaching 10^{-6}, the curve for the decline in rate is fairly well represented by the formula

$$y = 103.37 - 0.541\ (95.2 - x)^{1.18},$$

wherein y is the rate of nerve conduction, the rate in undiluted sea-water being 100 and x the concentration of the sodium, potassium, and calcium cations, their concentration in natural undiluted sea-water being 100.

Later as the OH ion concentration continued to decline the formula became

$$y = 100 - 0.2\ (100 - x)^{1.35},$$

and finally for the last 14 medusæ when the distilled water was still alkaline but nearly neutral, the rate is given by the formula

$$y = 2.512x^{0.8}.$$

This is also equally well represented by the formula

$$y = 100 - 0.778\ (100 - x)^{1.019}.$$

Thus the curves representing the rate of nerve-conduction in sea-water diluted with alkaline distilled water gradually approach and finally become identical with Freundlich's formula for absorption $y=ax^{1/n}$ when the distilled water becomes nearly neutral. In this case $a=2.512$ and $1/n=0.8$. The data are shown in detail in Table 1 and Figure 2 at the end of this paper.

Mecklenburg, in *Tables Annuelles Internationelles de Constants et données numérique*, **3**, 418 (1914), gives a list of 16 cases of adsorption in which a ranges from 0.0824 to 23.5 being usually between 2 and 3, while the exponent $1/n$ ranges from 0.167 to 0.965 being usually from 0.2 to 0.6. Hence 0.8 is high, but not beyond the range of the exponent in observed cases of adsorption.

Thus we venture to suggest that adsorption may play a fundamental rôle in nerve conduction, and that the only cations which are necessary to the reaction are the adsorbed sodium, calcium, and potassium ions, the rate of nerve conduction being proportional to the concentration of these adsorbed ions.

Hydroxyl and hydrogen ions are not adsorbed but act independently serving as accelerators of nerve conduction when in weak concentration and as depressants if concentrated to a greater degree.

A series of diagrams may serve to illustrate this hypothesis. Thus in Figure 1 the nerve is represented by a row of negative charged colloidal particles, for the colloid being normally alkaline the charge may be assumed to be negative.[1] Line No. 1 shows the nerve in its resting stage wherein the negative charge of each colloidal particle tends to be partially neutralized by the adsorbed cations of sodium, calcium, and potassium shown by + + +. The number of cations which each colloidal particle can capture and temporarily de-ionize[2] depends upon the potential of its negative charge, and also upon the concentration of the cations in the surrounding fluid. For the sake of illustration we have shown three such cations attracted to the surface of each particle, but in reality the number must be greater than this.

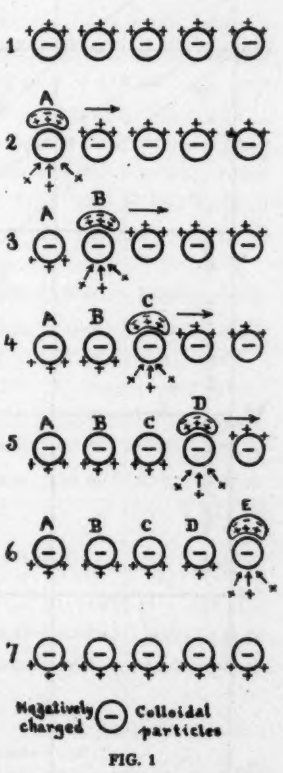

FIG. 1

Line No. 2 shows the beginning of a nerve impulse wherein the adsorbed cations of particle *A* have combined with some anions to form an ion-proteid, thus neutralizing their positive charges and unmasking the negative charge of the colloidal particle. As a result other cations from the surrounding fluid (sea-water) are at once attracted and captured by the particle.

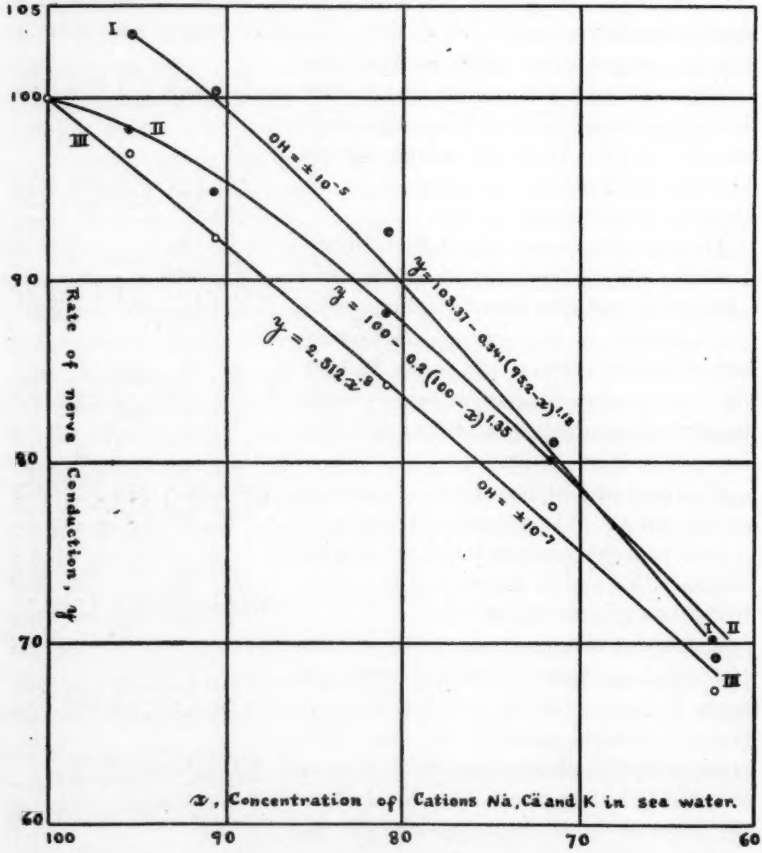

FIG. 2

In Line No. 3 the reaction has passed on to particle *B* and its negative charge is unmasked, and thus the negative charge passes through the nerve at the rate of nerve conduction until each particle has lost its original cations, and then recaptured others from the surrounding fluid as in Lines 2–7, Line 7 representing the resting nerve after the reaction

has passed through it. Thus by Wilhelmy's law the rate of nerve conduction is proportional to the concentration of the adsorbed sodium, calcium and potassium cations which conduct it.

Since 1899, Loeb[3] has maintained that physiological reactions are phenomena associated with the formation of ion-proteids, but I think that while this is true for nerve conduction it is only half the truth, and that it is possibly a phenomenon of *adsorption* combined with that of an ordinary chemical reaction.

TABLE I

SHOWING THE RATES OF NERVE-CONDUCTION IN CIRCUIT-SHAPED STRIPS OF SUBUMBRELLA TISSUE OF *Cassiopea xamachana* IN SEA-WATER DILUTED WITH DISTILLED WATER. EXPERIMENTS MADE AT TORTUGAS, FLORIDA, JULY 5-16, 1915.

	RATES OF NERVE-CONDUCTION					
	Natural sea-water $x=100$ (Z)	(Q) 95 s. w. +5 d. w. $x=95.2$	90 s. w. +10 d. w. $x=90.5$	80 s. w. 20 d. w. $x=80.8$	70 s. w. +30 d. w. $x=71.4$	60 s. w. +40 d. w. $x=62.2$
I. Observations of Medusæ Nos. 1-25 in sea-water diluted with alkaline distilled water containing somewhat less than 10^{-5} OH ion concentration, July 5-9, 1915. See curve I I, Fig. 2.....	100	103.37	100.33	92.67	81.09	69.33
Rate (y) calculated from the formula: $y=103.37-0.541\,(95.2-x)^{1.13}$		103.37	100.00	90.78	80.59	69.87
II. Observed rates of Medusæ Nos. 51-70, July 11-16 in sea-water diluted with distilled water which was less alkaline than in I.....	100	(+) 98.28 ±.96	94.86 ±1.18	88.19 ±1.83	80.12 ±1.61	70.3 ±1.96
Rate (y) calculated from the formula: $y=100-0.2\,(100-x)^{1.53}$.....						
See Curve II II, Fig. 2.....	100	98.24	95.52	88.59	80.27	70.92
III. Observed rates of Medusæ Nos. 58-71, July 14-16, 1915 in sea-water diluted with nearly neutral distilled water.....	100	96.86 ±.66	92.25 ±1.62	84.2 ±2.15	77.6 ±1.82	67.5 ±2.14
Rate (y) calculated from the formula: $y=2.512x^{0.8}$.....						
See curve III III, Fig. 2.....	100	96.15	92.32	84.27	76.38	68.40
The ratio $\frac{x^{0.8}}{\text{observed } y}$	0.398	0.395	0.397	0.398	0.397	0.403
Rate (y) calculated from the formula: $y=100-0.778\,(100-x)^{1.089}$	100	96.15	92.29	84 21	76.29	68.48

Q. 95 cc. sea-water + 5 cc. distilled water.

Z. $x = 100$ means that the concentration of the cations Na, Ca, and K in natural sea-water is taken to be 100. Then the concentration of the cations in 95 per cent sea-water is 95.2, etc.

+ Probable errors are stated as ± following a determination.

Loeb indeed is not antagonistic to the view that complex changes other than those of a simple chemical reaction may accompany nerve conduction, for he says, *Comparative Physiology of the Brain and Comparative Psychology*, p. 14 (1900): "We have to remember that all life phenomena are due to motions or changes occurring in colloidal substances."

Moreover Matthews,[4] 1902, states that protoplasm consists essentially of a colloidal solution, and stimulation is accompanied by the passing of this solution to or toward a gel; and with these statements I am in accord. Matthews, however, believed the anions to be the stimulating ions, and he also thought the colloidal particles carried a positive charge. Later studies by many observers have made it apparent that the cations are the active agents in most physiological processes, and that living protoplasm is normally alkaline and thus its colloids probably carry negative charges. Moreover the phenomena of adsorption were not well understood in 1902 and Matthews makes no mention of it in respect to nerve conduction.

No one indeed had reason to support the view that adsorption plays a part in nerve conduction until the determination of the change in rate of nerve conduction of *Cassiopea* in successive dilutions of sea-water suggested this as a possibility.

My results lend no support to the theory of Sutherland[5] that the velocity of propagation of nerve impulse is that of a shear in the substance of the nerve. If this were the case its rate would vary with the viscosity of the surrounding fluid, but the decline in rate is practically the same whether the sea-water be diluted with distilled water, 0.9 molecular dextrose, or 0.4 molecular magnesium chloride.

[1] Hardy, W. B., *J. Physiol., Cambridge*, **24**, 296 (1899).

[2] Bayliss, W. M., *Biochem. J.*, **1**, 177 (1906), finds that electrolytes when adsorbed are non-ionized and no longer take part in the electrical conductivity of the solution. See also: *Principles of General Physiology*, p. 54–71 (1915).

[3] Loeb, J., 1899, *Festschrift für Fick*, and *Amer. J. Physiol.*, **3**, 327–338 (1900).

[4] Matthews, A. P., *Science*, **15**, 496 (1902).

[5] Sutherland, W., *Amer. J. Physiol.*, **14**, 112 (1905), and *Ibid.*, **23**, 115 (1908).

ZUÑI CULTURE SEQUENCES

By A. L. Kroeber

MUSEUM OF THE AFFILIATED COLLEGES, SAN FRANCISCO

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The vicinity of the famous Indian pueblo of Zuñi in New Mexico has long been known to be rich in ruins. Many of these have been reported and described, some surveyed, and material from various sites has found its way into collections. A large body of specimens was secured through excavations by the Hemenway expedition, but this material and its data remain unpublished.

The region furnishes an unusual opportunity for an attack on the chronology, or at least the sequences of culture, in the prehistory of the

Southwest: first, because the restricted area excludes differences due to varying environments, and thus renders any observable distinctions directly interpretable in terms of time: second, because of numerous links between the historic and prehistoric periods. Several of the ruins were inhabited in Spanish times. They still bear native names that tally with those mentioned in sixteenth and seventeenth century records and some contain ruins of abandoned Catholic churches.

The tempting opportunity thus offered must of course be followed with the spade for ultimate results. I was in Zuñi during the summer of 1915. Pressure of ethnological work forbade digging; but some three thousand potsherds were gathered from the surface of about fifteen once inhabited sites within a few miles' radius of the pueblo. These were supplemented by a thousand fragments from the streets and roofs of Zuñi itself.

It was obvious that the pottery was of two well marked types, and that the surface of any one ruin yielded only such ware as plainly belonged to one or the other of the two classes. One set of sites is littered with sherds of which at least half are dull black or dark gray. The other half are as frequently red as white. Three-colored pieces—black and red on a white ground—are found. Corrugated ware is uncommon and about evenly distributed between dark and light.

On the second set of sites, black and red ware are both rare, white or whitish pieces constituting more than nine-tenths of the total. Three-color pottery has not been found. Corrugated sherds are common, but almost always of the light variety.

The first group of sites includes those which are mentioned as inhabited villages in the seventeenth century. Their sherds occur in nearly the same proportion as in modern Zuñi. These ruins therefore fall in part into the historic period. The second group of sites is wholly prehistoric. Their ware resembles that familiar as Cliff Dwellers' pottery. The two wares have been designated as type A, the later, and type B, the earlier.

The conditions of the ruins accords with this arrangement in time. Type A ruins normally include standing walls, and loose rock abounds. All type B sites are low or flat, without walls or rock, and show only pebbles in the surface soil. It seems more likely that this condition is due to the decay of age, or to the carrying away of the broken rock to serve as material in the nearby constructions of later ages, than to any habit of the period B people to build in clay instead of masonry. The latter possibility can be seriously entertained only if excavation reveals no building stone whatever in type B ruins.

Chips of obsidian are usually observable on period A sites, but have not been found on those of period B.

The proportions of different wares can be summarized thus:

	Eight Sites of Period A.	*Nine Sites of Period B.*
Wholly black	53	5
White or black on white	25	92
Containing any red	22	3
	100	100

Differences between sites of the same period can also be observed. These indicate minor periods of time. Expressed in percentages of the total number of sherds secured at each spot, the frequency of several wares is:

Period	*Site*	*Corrugated*	*Three Color*	*Black on Red*	*Any Red*	*Black*
Present	Zuñi	0[1]	12	1		
Late A	Towwayallanna	1	8	3	22	53
	Kolliwa	—	7	2		
	Shunntekya	2	7	2		
	Wimmayawa	2	4	1		
	Mattsakya	3	4	3		
	Kyakkima	4	3	2		
Early A	Pinnawa	10	1	8		
	Site W	24	—	1		
Late B	Hattsinawa	27	—	5	10	19
	Kyakkima West	12[2]	—	4	8	—[2]
Middle B	Shoptluwwayala	40	—	2	3	7
	Hawwikku B	49	—	6	12	9
Early B	Te'allatashshhanna	66	—	—	—	5
	Site X	71	—	—	3	1
	Tetlnatluwwayala	72	—	—	2	—
Uncertain	He'itli'annanna	—	—	—	—	3
	Site Y	—	—	—	—	—

[1] Present, but less than half of 1 %.

[2] Only 25 pieces altogether are available from this site.

The material as yet at hand is too slight, and too superficial in provenience, to make this classification into sub-periods more than tentative for any particular site. The statistics however do allow of three conclusions. First, the two principal periods are almost certainly subdivisible into shorter epochs. Second, these subdivisions shade into one another. Third, there is no gap or marked break between periods A and B. So far as Zuñi valley is concerned, the prehistory of Southwestern native civilization has therefore been in the main a continuous development from the earliest known time to the present.

A. V. Kidder's recent 'Pottery of the Pajarito Plateau,' in volume 2 of the *Memoirs of the American Anthropological Association*, presents analogous results, obtained by a method differing in some details, for another

region of New Mexico; and at San Cristobal in still another part of the state, N. C. Nelson has excavated a stratified deposit showing four successive layers of different type. It is quite likely that some of the types at these three sites will prove to be similar, or even identical, as soon as the material can be compared. In this event a chronological frame work would be established that may prove capable of extension to accommodate a considerable part of the prehistoric data from the Southwest, and to fix distinctive and otherwise undatable local variations of ancient culture. The impression that there were at least two principal periods in the Southwest, the earlier represented by what are currently called Cliff Dweller forms, has of course long been prevalent, but the supporting evidence has been random. The three present sequential determinations promise not only definitely to establish but to elaborate the older general conviction.

The findings here discussed will be published in the Anthropological Papers of the American Museum of Natural History.

THE NUMERICAL RESULTS OF DIVERSE SYSTEMS OF BREEDING

By H. S. Jennings

ZOOLOGICAL LABORATORY, JOHNS HOPKINS UNIVERSITY

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When organisms differing with respect to a pair of characters are bred for generation after generation, the relative numbers of individuals that will show any particular combination of characters in any given generation of course depend on the system of mating followed. The different classes of individuals may mate at random; or there may be assortative mating (dominants with dominants, recessives with recessives); or dominants alone may be bred; or recessives alone. Or again, self fertilization may prevail; or one of the various possible types of inbreeding may be followed. If we represent the two alternative characters by A (dominant) and a (recessive), then three types of individuals are possible, AA, Aa, and aa. The question here raised is as to the relative numbers of each of these three types of individuals after any number n of generations of mating, by any of the systems mentioned above. This depends of course on the constitution of the parents at the beginning, as well as on the system of breeding and the number of generations.

When breeding by a given system is continued for many generations, several types of results may be distinguished:

A. In some cases the proportions of the population having particular

constitutions (AA, Aa, or aa) remain the same for all generations. Examples are:

(1) Random mating when the parents are at the beginning either all Aa; or half AA, half aa; or when the parental proportions are AA+2Aa +aa. In these cases after any number of generations the resulting progeny show the proportions AA+2Aa+aa.

(2) Random mating, parental proportions, rAA to $t$aa. Constant proportion in offspring: r^2AA+2rtAa+t^2aa.

(3) Random mating, parental proportions, AA+Aa. Constant result, 9AA+6Aa+aa.

(4) Random mating, parental proportions, rAA+sAa. Constant result, $(s+2r)^2$AA+$2s(s+2r)$Aa+s^2aa.

(5) Inbreeding, daughters bred back to father: both parents Aa. Constant result, AA+2Aa+aa.

(6) Sex-linked factors; random mating; original parents rAA+rA− + $t$aa+ta−, in equal numbers. Constant later proportions: males, rA− +ta−; females, r^2AA+2rtAa+t^2aa. Males and females in equal numbers.

B. In some complex cases the population changes from generation, and the best we can do is to obtain a formula which shall, when we know the proportions in a given generation n, give us the proportions in the next following generation $n+1$.

Examples:

(7) Random mating, population in nth generation: rAA+sAa+$t$aa. Population in generation $n+1 = (s+2r)^2$AA + $2(s+2r)(s+2t)$Aa + $(s+2t)^2$aa.

(8) Assortative mating; population in the generation n is rAA+ sAa+$t$aa. In the next generation $(n+1)$ the population is $(2r+s)^2$AA+ $(4rs+2s^2)$Aa+$(s^2+4rt+4st)$aa.

(9) Selection of dominants; parental population in generation n is rAA+sAa+$t$aa. Population in the next generation $(n+1)$ is $(2r+s)^2$ AA+$2s(2r+s)$Aa+s^2aa.

C. In many cases the constitution of the population changes from generation to generation, as long as the given system of breeding lasts, and it is possible to give a simple formula in terms of n, the number of generations that the given system has been followed, from which formula the proportions in generation n can be directly computed.

Examples:

(10) Assortative mating (dominants with dominants, recessives with recessives); original parents are progeny of the cross AA × aa, so that their proportions are AA+2Aa+aa. After n assortative matings the proportions are: $(n+1)$AA+2Aa+$(n+1)$aa.

(11) Assortative mating; parental population is the result of the random mating of rAA+$t$aa. After n assortative matings the population is $r(r+nt)$AA+$2rt$Aa+$(n+1)t^2$aa.

(12) Assortative mating; parental population all Aa. After n assortative matings the population is nAA+2Aa+$n$aa.

(13) Assortative mating; parental population AA and Aa in equal numbers. After n matings the population is $(3n+6)$AA+6Aa+$n$aa.

(14) Selection of dominants; parental population AA+2Aa+aa. After n generations the population is $(n+1)^2$AA + $2(n+1)$Aa + aa.

(15) Self-fertilization; parents all Aa. After n self-fertilizations the population is (2^n-1)AA + 2Aa + (2^n-1)aa.

(16) Self-fertilization; population at the beginning, rAA + sAa + $t$aa. After n generations the population is $[r(2^{n+1})+s(2^n-1)]$AA + $2s$Aa + $[t(2^{n+1})+s(2^n-1)]$aa.

D. In most cases the constitution of the population changes from generation to generation, giving a series of values not readily expressed in terms of n (the number of generations) alone. In these cases diverse systems or diverse parents give different series of values, almost all of which are examples of certain simply derived mathematical series, or of their combinations. The results are therefore best presented by giving first these fundamental series, each with its designation. Then the results of any number of generations of any system of breeding can be given by designation of the series which it forms. The main series, with their first 10 terms, are given in the following table:

Table of the Fundamental Series in Mendelian Breeding

SERIES	HOW FORMED												
n	x+1	0[1]	1	2	3	4	5	6	7	8	9	10	
B	2x	1	2	4	8	16	32	64	128	256	512	1024	$(=2^n)$
C	2x+1	0	1	3	7	15	31	63	127	255	511	1023	$(=2^n-1)$
D	2x−1		2	3	5	9	17	33	65	129	257	513	$(=2^{n-1}+1)$
E	2x+1			2	5	11	23	47	95	191	383	767	$(=2^n-2^{n-2}-1)$
F	Sum of two preceding	0	1	1	2	3	5	8	13	21	34	55	
G	2x+1, then 2x−1	0	1	1	3	5	11	21	43	85	171	341	$(=B_{n-1}-G_{n-1})$
H	G−F	0	0	0	1	2	6	13	30	64	137	286	
I	B−G−F	1	0	2	3	8	16	35	72	150	307	628	$(=G_{n+1}-F_n)$
J	B_n-F_{n+1}	0	1	2	5	11	24	51	107	222	457	935	
K	B_n-F_{n+2}	0	0	1	3	8	19	43	94	201	423	880	
L	$B_n-F_{n-1}-G_{n-1}$		2	2	6	11	24	48	99	200	406	819	
M	$2x+F_{n-1}$	2	4	9	19	40	83	171	350	713	1447	2952	$(=3B_n-F_{n+2})$

The table gives in the first column a letter by which the series will be designated (as n, B, C, J, etc). In the next column is given the method by which any given term of the series is formed from the preceding term, this preceding term being designated x. Thus, under C, the expression, $2x+1$ means that to obtain any term of the series, we double the preceding term and add 1. In some of the later series the derivation here given shows that the given series is formed by some combination of two previous series. Thus, series H is formed by substracting from any term of G the corresponding term of F. Any given term of a series is designated by a subscript representing the corresponding term of the series n: thus, F_4 means the fourth term of the series F (it is therefore 3); J_n means the nth term of the series J; D_{n+1} means the $(n+1)$th term of D; etc. At the end of each series is given certain other information; showing as a rule how any term of the series may be otherwise designated; or another method of derivation.

The results of any system of breeding that falls in the present group may be given by specifying which series of the table it gives and what terms of the series correspond to a given generation. Examples will make this clear:

(17) Self-fertilization, beginning with a cross between AA and Aa. After n self-fertilizations the proportions of the resulting population are given by the following:

$$AA = \frac{E_{n+2}}{B_{n+2}}. \qquad Aa = \frac{1}{B_{n+1}}. \qquad aa = \frac{C_n}{B_{n+2}}.$$

This signifies that for any generation n, the proportion of AA will be found by dividing the term $(n+2)$ of the series E by the term $(n+2)$ of the series B. So, after one self-fertilization the proportion that are AA will be 5/8; after two, 11/16; after eight, 767/1024. After five generations the proportion that are Aa will be 1/64; of aa, 31/128; and so on.

(18) Self-fertilization in a typical Mendelian population composed of AA+2Aa+aa. After n generations:

$$AA = \frac{C_{n+1}}{B_{n+2}}. \qquad Aa = \frac{1}{B_{n+1}}. \qquad aa = \frac{C_{n+1}}{B_{n+2}}.$$

(19) Inbreeding, brother by sister, in the progeny derived from a cross of AA by aa. After n generations:

$$AA = \frac{J_n}{B_{n+1}}. \qquad Aa = \frac{F_{n+1}}{B_n}. \qquad aa = \frac{J_n}{B_{n+1}}.$$

(20) Inbreeding, brother by sister, when the original cross was AA by Aa. After n inbreedings:

$$AA = \frac{M_{n+1}}{B_{n+3}}. \qquad Aa = \frac{F_{n+3}}{B_{n+3}}. \qquad aa = \frac{K_{n+1}}{B_{n+3}}.$$

(21) Inbreeding, parent by offspring; parents AA by Aa; progeny mated back continually to Aa:

$$AA = \frac{D_{n+1}}{B_{n+2}}. \qquad Aa = \frac{1}{2}. \qquad aa = \frac{C_n}{B_{n+2}}.$$

(22) Sex-linked characters; a cross between AA and a−, then breeding by random mating. After n random matings:

Males: $A- = \frac{G_{n+1}}{B_n}$: thus, 1/2, 3/4, 5/8, etc.

$a- = \frac{G_n}{B_n}$: thus, 1/2, 1/4, 3/8, etc.

Females: $AA = \frac{G_n \times G_{n+1}}{B_{2n-1}}$; thus 1/2, 3/8, 15/32, 55/128, etc.

$aa = \frac{G_n \times G_{n-1}}{B_{2n-1}}$; thus 0, 1/8, 3/32, 15/128, etc.

$Aa = 1 - AA - aa$; thus 1/2, 4/8, 14/32, 58/128, etc.

Males and females equal in number.

(23) Sex-linked characters; a cross between AA and a−, then inbreeding, brother by sister. After n inbreedings:

Males: $A- = \frac{G_{n+1}}{B_n}$. $a- = \frac{G_n}{B_n}$.

Females: $AA = \frac{I_{n+1}}{B_{n+1}}$. $Aa = \frac{F_{n+1}}{B_n}$. $aa = \frac{H_{n+1}}{B_{n+1}}$.

(24) Sex-linked characters; cross between AA and a−, then inbreeding, daughters mated to fathers, sons to mothers (as could be carried out only in certain lower organisms). After n inbreedings:

Males: $A- = \frac{G_{2n+1}}{B_{2n}}$: thus, 3/4, 11/16, 43/64, etc.

$a- = \frac{G_{2n}}{B_{2n}}$: thus, 1/4, 5/16, 21/64, etc.

Females: AA $= \frac{G_{2n+2} - 3^{n-1}}{B_{2n+1}}$; thus 4/8, 18/32, 76/128, etc.

Aa $= \frac{3^{n-1}}{B_{2n}}$; thus 1/4, 3/16, 9/64, etc.

aa $= \frac{G_{2n+1} - 3^{n-1}}{B_{2n+1}}$; thus 2/8, 8/32, 34/128, etc.

Males and females equal in number.

In all these series the proportions of any given sort gradually approach a limit; thus in the last case (24) the limit for A− is 2/3; for a−, 1/3; for AA it is 2/3; for aa, 1/3; for Aa the limit is 0.

In the full paper there is a systematic presentation of formulae for the proportions in any generation, resulting from any of the main types of breeding, and with any of the common types of parental population, giving eighty-two numbered sets of formulae. In each case the limit approached is given, together with the number of generations of breeding required to come within 1% of that limit. The complete paper appears in the first number of *Genetics*.

ON THE EFFECTS OF FEEDING PITUITARY BODY (ANTERIOR LOBE) SUBSTANCE, AND CORPUS LUTEUM SUBSTANCE TO GROWING CHICKS

By Raymond Pearl

BIOLOGICAL LABORATORY, MAINE AGRICULTURAL EXPERIMENT STATION

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In connection with the studies of Pearl and Surface[1] on the effect of pituitary substance on the function of egg production in the domestic fowl it seemed desirable to see whether the *initial* activation of the ovary could be accelerated by means of this substance. A pullet of a good producing strain hatched at the proper time and well grown will begin to lay when from five to six months of age ordinarily. Can such pullets be brought to sexual activity and laying any earlier if regularly fed pituitary substance?

Forty-five pure-bred Barred Plymouth Rock pullets, all hatched the same day (April 29, 1915) were divided into three lots of 15 each. They were so chosen that the total weights of the three lots, and thus the average weight per bird, were identical at the beginning of the experiment. Further great pains were taken to get birds of the same stage of maturity and physiological development so far as could be determined. Each bird in one lot (A) received *per os* 0.082 g. per day of pituitary body

(anterior lobe) substance. This was administered in No. 2 gelatine capsules with lactose as a diluent. Each bird in the second lot (B) received *per os* the same amount (0.082 g.) per day of dessicated corpus luteum substance from pregnant cows, again administered in No. 2 gelatine capsules with lactose as a diluent. Finally lot C was a control, the birds receiving no capsules. All the birds were housed in the same house, and given the same food and care except for the capsule feeding as above noted.

The experiment began August 13, 1915. The growth of the birds is shown in the table.

Table showing the weights at various intervals of birds fed pituitary body (anterior lobe) substance and of those fed corpus luteum substance.

DATE	LOT A. (PITUITARY)		LOT B. (CORPUS LUTEUM)		LOT C. (CONTROL)	
	Total weight	Mean weight per bird	Total weight	Mean weight per bird	Total weight	Mean weight per bird
	kilos	*grams*	*kilos*	*grams*	*kilos*	*grams*
Aug. 13. Initial weight...	16.78	1112.2	16.78	1112.2	16.78	1112.2
Aug. 28.....	22.09	1472.6	21.91	1460.6	22.14	1475.7
Sept. 4.....	23.81	1578.6	23.59	1571.1	24.18	1611.8
Sept. 11.....	24.68	1645.0	24.22	1614.8	25.40	1693.4
Sept. 18.....	25.72	1714.6	25.45	1696.4	26.81	1787.2
Sept. 25.....	27.12	1808.3	25.63	1708.5	28.26	1883.9

From this table it appears that:

1. Both pituitary body (anterior lobe) substance and corpus luteum substance retarded the growth of the birds in this experiment. Throughout the experiment all of the birds were in a perfectly healthy and active condition. It was impossible to detect any differences in these respects between the three lots. There appears to be no means of accounting for the growth differences observed except as a result of the organ substances fed.

2. The retardation of growth in the case of the birds fed corpus luteum substance (lot B) is greater than that in the lot fed pituitary substance (lot A). The differences are very small and not separately significant until the September 11 and later weighings, from which time they are relatively large.

3. After 43 days of organ substance feeding of birds starting at the same body weight those birds fed pituitary substance average 4.01% less in body weight, and those fed corpus luteum substance average 9.31% less in body weight, than normal controls.

A parallel experiment to that reported above has been run with male birds, and gave essentially the same results. Since the numbers used were too small to give reliable figures, they are not published.

Leaving now the matter of growth in body weight we may turn to a consideration of the attainment of sexual maturity in these three lots of birds. There was practically no difference between the three lots of birds in respect to time of beginning laying. The small differences exhibited cannot be regarded as significant, considering the nature of the character.

Several interesting points are brought out by this experiment. In the first place the results with pituitary body substance confirm all of our earlier work with this substance so far as concerns activation of the ovary. There is no evidence that the administration of pituitary substance hastened in any way the initial activation of the pullet ovary. The pituitary birds did not in fact begin to lay as soon as those receiving corpus luteum substance, though the two day difference between the lots cannot be regarded as significant. We have now tested the effect of pituitary (anterior lobe) substance on the ovary in three different physiological states, viz., (*a*) completely resting adult ovary during moult, (*b*) adult ovary in laying condition but with declining fecundity[2] rate, and (*c*) the inactive, immature ovary of the young pullet. In none of these physiological states has there been the slightest evidence that the pituitary has activated or accelerated the activity of the ovary in any manner or degree.

In the second place it appears that both pituitary (anterior lobe) substance and corpus luteum substance retard growth in the chick, but without affecting the attainment of sexual maturity (egg laying). The pullets in lots A and B began to lay at the same time they would have had they not received organ substance, but their body weight at the onset of laying was from 4 to 9% smaller than it would normally have been. These results are of interest in connection with the experiments of Gudernatsch[3] in feeding thyroid and thymus substance to growing tadpoles, though there is no evidence in the present experiments of differentiation being accelerated. It merely is *not* retarded, while body growth *is* retarded by pituitary and corpus luteum substance. It will be a matter of much interest to extend the period of feeding these substances, particularly corpus luteum, into earlier life. I propose to do this next year. For students of growth a means is afforded in corpus luteum feeding of notably retarding body growth without disturbing, so far as yet appears, the normal physiology or physiological development.

The complete paper describing these results and discussing them further will shortly be published elsewhere.

[1] Pearl and Surface, *J. Biol. Chem.*, 19, 263–278 (1914); and *Ibid.*, 21, 95–101 (1915).

[2] Pearl, R., *J. Biol. Chem.* (*in press*, 1916).

[3] Gudernatsch, J. F., *Arch. Entw.-Mech.*, 35, 457 (1912); also *Amer. J. Anat.*, 15, 431–478 (1914).

A PRELIMINARY REPORT ON FURTHER EXPERIMENTS IN INHERITANCE AND DETERMINATION OF SEX

By Richard Goldschmidt
(Of the Kaiser Wilhelm Institut für Biologie, Berlin)
OSBORN ZOOLOGICAL LABORATORY, YALE UNIVERSITY
Received by the Academy, November 29, 1915

In two former papers[1] the interesting 'gynandromorphism' produced by crossing the European and the Japanese races of the gipsy-moth (Lymantria dispar) was described and an experimental analysis of the phenomenon was attempted. From the data obtained important conclusions on the sex-problem in general were drawn. Although the main points seemed to be clear, a series of questions still remained open. One of them was, that the (apparently) same kind of crosses did not give the same results, if the material used had a different origin. It could be regarded as practically certain that the chief result, viz., the appearance of gynandromorphism in certain crosses found its right explanation in the hypothesis of a quantitatively different behavior or a different potency of the male sex-factors in the different races. Some of the experiments led me to suspect that this potency varied with the geographical distribution of the moth. Therefore it was one of my aims during a sojourn in Japan, to study the behavior of different local forms in that country in various crosses *inter se* and with different European forms.[2] These experiments are in no way to be regarded as completed, but the results so far obtained are so interesting and seem to bring the definitive solution of the problem so near, that a preliminary report on a part of them may be made.

A few words are first needed about the terminology. In previous papers I have used the term of gynandromorphism to indicate the sexual abnormalities produced in the racial crosses of these moths. It seems, however, no longer advisable to use this term, as it is applied more or less generally to quite a different phenomenon, *i.e.*, to individuals showing a mosaic of the characters of both sexes. In such a gynandromorph—see for example Boveri's late analysis of the Eugster bees[3]—a given organ or complex of cells is either male or female. But this

is not the case with my moths; the entire individual represents a definite, quantitatively fixed stage intermediate between the two sexes. If we represent femaleness and maleness as the end points of a series, say one as 0 and the other as 100, a given specimen of these 'gynandromorphic' moths would be represented by the points 12 or 35, etc. These animals do not represent a mixture of the characters of the two sexes, but a definite point between the two extremes, maleness and femaleness. In the former papers some characters were thought to be a mosaic, viz., the color of the wings, which shows in certain stages definite patches of male and female design. It is now clear that this is but a consequence from the physiology of pigmentation, a fact that later will be the starting point for important theoretical discussions. So it seems advisable to introduce a new term to designate the phenomenon treated in these experiments. I shall call in the future the different sexual intermediates *intersexes;* female intersexes, if they are genetically females, but transformed to some stage towards maleness, and male intersexes if the transformation goes in the opposite direction; the whole phenomenon being called intersexualism.[4]

Before describing the new results a short account might be given of the different stages of intersexualism so far produced in the experiments. Every single step has now been bred from a normal female through the different grades of female intersexes to a normal male; also the steps starting from the normal male through male intersexes towards the female up to three-fourths of the way. Every single one of these steps can be produced now at will by crossing the right combination of races. Female intersexualism begins with animals, which show feathered antennae of medium size (feathered antennae are a male character) but which are otherwise entirely female in appearance except that they produce a smaller number of eggs, which are fertilized normally. In the next stage patches of the brown male pigment appear on the white female wings, in steadily increasing quantity. The instincts are still female, the males are attracted and copulate. But the characteristic egg sponge laid by the animal contains nothing but anal hairs, in spite of the fact that the abdomen is filled with ripe eggs. In the next stage whole sections of the wings show male coloration, with cuneiform female sectors between, the abdomen becomes smaller, contains fewer ripe eggs, the instincts are only slightly female, the males are attracted very little, and reproduction is impossible. In the next stage the male pigment covers practically the whole wing, the abdomen is almost male, but still contains ovaries with a few ripe eggs, the instincts are intermediate between males and females. Then follow very male-like animals, which still

show in different organs their female origin and have rudimentary ovaries. They lead to practically male-like animals, called 'female-males' in my former paper, containing gonads which exhibit all transitional stages from an ovary to a testis. The end of the series is formed by males, which show in some minor characters, such as the shape of wings, still some traces of their female origin.

The series of the male intersexes starts with males showing a few white female spots on their wings. These become larger and larger, the amount of brown pigment correspondingly decreasing, finally (in extreme specimens) leaving only a few granules along the veins. Hand in hand with this the abdomen increases in size, reaching in the most extreme case two-thirds of the female size (without containing eggs!). The same is true for the instincts, which become more and more female. Further, the copulatory organs, which show in the female intersexes also a complete series of gradations from femaleness to maleness, and finally in the testis, which shows the first steps of transformation into an ovary.

There is another new point to be mentioned in this connection, as the fact that it was unknown is probably responsible for some errors in my former papers. It has been noticed that there are Japanese races, which contain a wing-pigmentation factor in the female sex; and the effect produced can be increased in certain crosses. In this way females with pigmented wings may arise, which may be mistaken for intersexes, and if this phenomenon occurs together with slight intersexualism, low grade intersexes may be mistaken for high grade ones. The further investigation of this will be of importance.

Going over now to the breeding results, we shall describe first only those relating to the female intersexes. It was found that there are in the European as well as in the Japanese forms, definite races possessing a special potency of the male sex-factors. If we cross forms of like or similar potency, the offspring is normal. But if we cross races with different potency of the male factors we get in F_1 female intersexes, provided the mother belongs to the race of lower potency. The degree of intersexuality depends of course upon the difference of the potencies. The European races so far known to me all show low potency; and in the Japanese races all grades are found from the lowest to the highest potency of those factors. If now any European race is crossed in any direction with one of the 'weak' Japanese races (the races K, Fu, M, H of my records) only normal females appear in the offspring. The same naturally holds true for crosses of any of these forms *inter se*. But all European females and also the females of the weak Japanese races,

crossed with males of the 'strong' Japanese races (*i.e.*, G, O, A) ought to yield female intersexes, the grade of intersexuality depending upon both races involved in the cross. In detail the main results are the following; the F_1 males being left out of account:

1. All possible F_1 combinations between the weak Europeans S and F and the weak Japanese K, Fu, M, H contain only normal females.

2. Males of the moderately strong Japanese race G crossed with the very weak European females F produce nothing but high grade female intersexes ranging almost up to the above quoted 'female-males.' The same males produce, if crossed with the somewhat less weak European race S, nothing but medium female intersexes. The same males again crossed with the still less weak Japanese race H give in F_1 low grade female intersexes; and finally from the cross between these males and the highest of the weak Japanese races, K, we get only slight female intersexualism. There is to be added that the intersexuality affects every single individual that is genetically (one X-chromosome) a female.

3. Another of the Japanese races, which has a higher potency than G, produces in similar crosses with the weak European races the highest grade of female intersexuality, the so-called 'female-males.'

4. If the males used in the crosses belong to the strongest known Japanese races A and O (there is only a slight difference between these two which may be neglected here) and the females to the weak European races F and S, or the Japanese race H, all individuals genetically being females become transformed entirely into males. It may be added here that all results are based on fair numbers. For instance for this cross there are about 14 cultures with over 1000 individuals. In many cultures mortality is exactly recorded.

5. The following is to be regarded as an *experimentum crucis* for the correctness of the explanation given above: The Japanese race K proved to have a low potency of the male sex-factors, as crosses with European females produced normal offspring. Furthermore the same race K proved to be somewhat higher in potency than the European races by producing only the slightest degree of intersexuality in crosses with other males of the medium strong race G, whereas the European races gave in the corresponding cross with G-males up to high-grade intersexes. These same Europeans produce in the crosses with the strong races A and O nothing but males. Here we have an equation from which we should expect that in a cross between females from the races K and males A or O medium to high-grade intersexes will be produced. This is the actual result.

6. To these results may be added the following details:

a. The results are typical and, so far, without exception. They seem to be independent of external conditions as the same results were obtained identically (besides my former experiments carried on in Munich) in my cultures at the Bussey Institution, in Boston[5] and those of my assistant Mr. Seiler in Berlin.

b. The grade of intersexuality in one culture is subject to a regular and continuous variation around a mean. If we divide the distance between femaleness and maleness in 100 grades a given culture would show a symmetric variation of a certain range, say 20 grades, around a mean of 50 or 60, etc. What causes certain differences in the position of the modal class in sister cultures is not yet clear, but will be very important for the theoretical side of the question. It is to be expected that at the two end points of the series the individuals overlap on the normal, *i.e.*, respectively normal females and males. This was actually found in the border cases. If all individuals which are genetically females are changed into males some minus individuals may be recognizable by their somewhat female shape of the wings, proving that the variability extends into the normal, too. It is to be hoped that the statistical treatment of these facts, together with the F_2 results and the data regarding the male intersexes will open a way to an exact calculation of the relative values of potency.

c. In the crosses producing nothing but males, occasionally there appears a single normal female, hatching as the last individual of a given culture. It is very probable that we face here another case of non-disjunction (Bridges). A spermatozoon without X-chromosome ought to give a normal female with every egg (assuming naturally that the male factors are carried in the X-chromosomes). So far I have succeeded only in finding a single spermatocyte II with 30 instead of 31 chromosomes.

So far only the facts regarding the female intersexes have been recorded, and they form a strong support for the views of my earlier papers in regard to the sex-problem. But the new results about the male intersexes show that in one important point the hypotheses have to be changed. I stated there that the male intersexes appear in F_2 from the reciprocal cross, not giving female intersexes in F_1. The fact that exactly one-eighth of the males were intersexual corroborated strongly the Mendelian formula of sex-inheritance used by me, working with two pairs of sex factors. The new results give a different aspect to the facts:

1. The appearance of the male intersexes in the said crosses is a single event conditioned by the two races involved in the cross. The ratio $\frac{1}{8}$ is typical, too, only for the special combination. In other crosses any ratio between 0 and 50 percent may be typical.

2. A new fact of decisive importance is that male intersexes may be produced in F_1. So there were a few individuals in the crosses female K $\times$ male S and female O $\times$ male H. And in the cross: weak Japanese female K $\times$ weak Japanese male H, nothing but intersexual males appear in F_1.[6] These facts are of greatest importance for the whole question. Together with some other F_2 results and the insight in some former errors of interpretation they make it very probable that the female part of the formulae F F M m = ♀

F F M M = ♂ does not mendelize but is inherited plasmatically. This part too shows different potencies and the final result is the combined effect of both groups. However, it seems undesirable to insist on this point, as the decisive experiments, which may bring the complete solution, have not yet been accomplished.

[1] Goldschmidt, R., Erblichkeitsstudien an Schmetterlingen. I., *Zs. induct. Abstammungslehre.*, 7, (1912); and Goldschmidt, R., und Poppelbaum, H., idem. II. *Ibid.*, 11 (1914)

[2] The existence of such local forms could be shown by breeding experiments. The problem of the geographical races of this moth has been for many years the object of my principal studies. However, the results are not yet ripe for publication.

[3] Boveri, Th., Ueber die Entstehung der Eugsterschen Zwitterbienen., *Arch. Entw-Meck., Leipzig*, 41 (1915).

[4] The fact that male and female intersexes are different calls certainly for an explanation, but will not be discussed here. It may only be said that it is a question of the physiology of development.

[5] I wish to express my sincerest thanks to Professor Wheeler and all the members of the staff of the Bussey Institution, Harvard University, for their kindness in granting me the facilities for prosecuting my work.

[6] A similar cross with the same race H but another mother has been carried out already with the same result by Toyama's assistant, Dr. Machida, to whom therefore belongs the priority of this discovery. I am indebted to him, too, for the material of the race H. I do not know whether he has meanwhile published this result.

ON THE DEGREE OF INBREEDING WHICH EXISTS IN AMERICAN JERSEY CATTLE

By Raymond Pearl and S. W. Patterson

BIOLOGICAL LABORATORY, MAINE AGRICULTURAL EXPERIMENT STATION

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In a series of 'Studies on Inbreeding' published during the past few years Pearl[1] has described in detail a method whereby it is possible to measure exactly the degree of inbreeding which exists in the pedigree of any particular individual animal. This is done by means of coefficients of inbreeding. These quantities may be defined as follows:

In the genetic passage from the $n + 1$'th ancestral generation to the n'th, or in other words the contribution of the matings of the $n+1$'th generation to the total amount of inbreeding involved in the production of an individual, the degree of inbreeding involved will be measured by the expression

$$Z_n = \frac{100\,(p_{n+1} - q_{n+1})}{p_{n+1}} \qquad (1)$$

where p_{n+1} denotes the maximum possible number of different individuals involved in the matings of the $n+1$ generation, and q_{n+1} the *actual* number of different individuals involved in these matings. Z_n may be called a *coefficient of inbreeding*. If the value of Z for successive generations

in the ancestral series be plotted to the generation number as a base, the points so obtained will form a curve which may be designated as the *curve of inbreeding*.

It will be noted that the coefficient of inbreeding Z is the percentage of the difference between the maximum possible number of ancestors in a given generation and the actual number realized. The coefficient may have any value between 0 and 100. When there is no breeding of relatives whatever (that is, in the entire absence of inbreeding) its value for each generation is 0. As the intensity of the inbreeding increases the value of the coefficient rises.

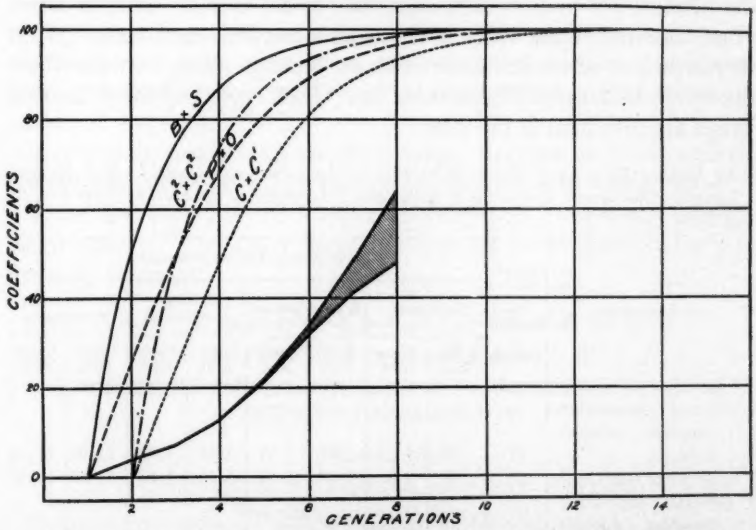

FIG. 1.—DIAGRAM SHOWING THE INBREEDING CURVES FOR A RANDOM SAMPLE OF THE GENERAL POPULATION OF AMERICAN JERSEY *COWS*. THE TWO HEAVY LINES GIVE THE UPPER AND LOWER LIMITING VALUES FOR THE SUCCESSIVE MEAN INBREEDING COEFFICIENTS. THE TRUE VALUE OF THE CURVE LIES SOMEWHERE IN THE RULED AREA BETWEEN THESE HEAVY LINES. FOR COMPARISON THE CURVES FOR CONTINUED BROTHER × SISTER (B × S), PARENT × OFFSPRING (P × O), AND FIRST COUSIN × FIRST COUSIN BREEDING ARE INCLUDED. $C^1 \times C^1$ DENOTES SINGLE COUSIN MATINGS, AND $C^2 \times C^2$ DOUBLE COUSIN MATINGS.

It is obvious that this method provides for the first time a means of examining statistically the degree of inbreeding which exists on the average in a population, or in any differentiated group of a population. It is the purpose of the present note to give in a very brief and preliminary way certain of the results of such a statistical examination of inbreeding in American Jersey cattle.

The first step in the treatment of the problem consisted in working out complete pedigrees to and including the eighth ancestral generation of 254 individual animals registered in the Herd Book of the American

Jersey Cattle Club. These animals were divided into four series as follows: (*a*) a random sample of the general population of males, (*b*) a random sample of the general population of females, (*c*) a selected sample of Register of Merit males, so chosen as to include the leading bulls in the recent history of the breed, these being bulls whose daughters were conspicuously excellent producers of milk and butter fat, (*d*) a selected sample of Register of Merit females so chosen as to include all of the cows which have recently made world's records in respect of butter fat production.

For reasons which cannot be gone into here, but will be presented in the detailed publication, it is impossible in the case of cattle to arrive at an absolutely exact value of the mean inbreeding coefficients. What we can do is to determine upper and lower limiting values, between which the true, and undeterminable value lies. Such upper and lower limiting values are presented in the table.

Table Showing Mean Coefficients of Inbreeding for American Jersey Cattle, for Both Random Samples of the General Population of Both Sexes and Samples of the Animals in the Register of Merit.

CLASS OR GROUP	NO. OF PEDIGREES INCLUDED IN SAMPLE	MEAN COEFFICIENTS OF INBREEDING							
		Z_0 Parents		Z_1 Grandparents		Z_2 Great-grandparents		Z_3 Great-great-grandparents	
		Lower limit	Upper limit	Lower limit	Upper limit	Lower limit	Upper limit	Lower limit	Upper limit
1. General population (random sample) bulls	67	0	0	1.87	1.87	5.41	5.41	11.01	11.21
2. Register of Merit bulls	63	0	0	1.59	1.59	5.36	5.62	14.88	15.91
3. General population (random sample) cows	61	0	0	4.10	4.10	6.97	6.97	12.50	12.59
4. Register of Merit cows	63	0	0	0	0	2.98	2.98	9.23	9.42

CLASS OR GROUP	NO. OF PEDIGREES INCLUDED IN SAMPLE	MEAN COEFFICIENTS OF INBREEDING							
		Z_4 Great-great-great grandparents		Z_5 (Great)[4] grandparents		Z_6 (Great)[5] grandparents		Z_7 (Great)[6] grandparents	
		Lower limit	Upper limit	Lower limit	Upper limit	Lower limit	Upper limit	Lower limit	Upper limit
1. General population (random sample) bulls	67	19.59	20.79	29.48	32.77	39.14	47.93	46.07	63.12
2. Register of Merit bulls	63	23.32	28.39	30.28	43.99	34.80	62.03	37.07	77.77
3. General population (random sample) cows	61	21.26	21.89	31.53	33.82	41.03	48.18	47.56	63.10
4. Register of Merit cows	63	15.33	16.27	24.63	28.53	33.12	44.06	39.22	61.45

From this table we get, for the first time, a definite quantitative conception of the average degree of inbreeding existing in any animal. In the complete publication which will shortly follow this note additional details will be given, including probable errors and other statistical constants.

From this analysis of Jersey inbreeding records two results, among others, stand out with particular clearness and significance. These are:

1. That American Jersey cattle, at the present time, may be said in general and on the average to be about one-half as intensely inbred, when account is taken of the eighth ancestral generation, as would be the case if continued brother × sister breeding had been followed. The *form* of the inbreeding curve is, however, very different in the two cases, the brother × sister curve being concave to the base line throughout, while the actual Jersey curves tend to have their principal curvature convex to the base.

2. That, in general and on the average, Register of Merit animals are *less* intensely inbred than the general population of Jersey cattle.

A detailed report of the work on inbreeding in Jerseys will be published shortly. The above notes are intended to be merely of a preliminary character.

[1] A list of references to these papers will be found in *Amer. Nat.*, **48**, 513 (1914).

UPPER LIMIT OF THE DEGREE OF TRANSITIVITY OF A SUBSTITUTION GROUP

By G. A. Miller

DEPARTMENT OF MATHEMATICS, UNIVERSITY OF ILLINOIS

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In a recent article published in the *Bulletin of the American Mathematical Society* (**22**, 68, 1915) I proved the following theorem: *A substitution group of degree* $n = kp + r, p > k$ *and* $r > k$, *which is neither alternating nor symmetric cannot be more than* r *times transitive unless* $k = 1$ *and* $r = 2$. From this theorem and the well known theorem that there is at least one prime number in the interval m (exclusive) and $2m - 2$ (inclusive), whenever $m \geq \frac{7}{4}$, it results directly that the degree of transitivity of a substitution group of degree n which does not include the alternating group of this degree must be less than $3\sqrt{n} - 2$. In the article noted above the condition $n > 12$ was added in stating this result but this condition can clearly be omitted.

The general expression $3\sqrt{n} - 2$ for an upper limit of the degree of transitivity of a substitution group of degree n which does not include

the alternating group of this degree gives clearly a much smaller value for this upper limit, when n becomes large, then the expression $\frac{n}{3}+1$, which is found in the standard works of reference, for instance, in Pascal's *Repertorium der höheren Mathematik*, volume 1, (1910), page 211. It may therefore be of interest to note that it is possible to deduce from the italicized theorem noted above a general expression which gives still smaller values for the upper limit in question.

In fact, if p is a prime number in the interval $\sqrt{n}$ (exclusive) and $2\sqrt{n}-2$ (inclusive) then r can always be so selected that it does not exceed $p+n/p$. Since this expression is an increasing function of p when $p>\sqrt{n}$ and has the value $\frac{5}{2}\sqrt{n}$ when $p=2\sqrt{n}$, it results that the prime number p can be so chosen that r is always less than $\frac{5}{2}\sqrt{n}-1$. That is, the degree of transitivity of *a substitution group of degree n which does not include the alternating group of this degree is always less than* $\frac{5}{2}\sqrt{n}-1$. For all integral values of $n>43$ this theorem gives a smaller upper limit for the degree of transitivity than the formula $\frac{1}{3}n+1$, and for all values of $n>4$ it gives a smaller upper limit than $3\sqrt{n}-2$.

THE EXTENSION OF THE MONTANA PHOSPHATE DEPOSITS NORTHWARD INTO CANADA

By Frank D. Adams and W. J. Dick

COMMISSION OF CONSERVATION OF CANADA

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With the development of agriculture in Western Canada there will arise in the not distant future an insistent and ever increasing demand for fertilizers and manures. Phosphoric acid is one of the most important constituents of such fertilizers. While no deposits of phosphates have hitherto been found in Western Canada, great beds of phosphate of lime have in recent years been discovered in Utah, Idaho, Wyoming, and Montana.

It was for the purpose of ascertaining whether these phosphate-bearing rocks extend northward into Canada that the present investigation was undertaken by the Commission of Conservation of Canada.

The phosphate beds from Utah to Montana occur at a certain definite horizon, usually known as the Phosphoria Formation, which is found near the summit of the Pennsylvanian subdivision of the Carboniferous system. This Pennsylvanian is usually composed of four formations, the phosphate lying at the base of the highest of these members and being usually associated with bands of chert.

The phosphate deposits in the United States which are nearest to the Canadian line are those which are found near Helena, Montana. Further north the great Louis overthrust comes in carrying the Cambrian and Pre-Cambrian over the Carboniferous, which, however, reappears at the Canadian boundary in the Flathead country.

The Carboniferous in Western Canada is practically confined to the Eastern slope of the Rocky Mountains in Alberta and the adjacent parts of British Columbia.

An examination of these mountains was, therefore, made along three transverse lines of sections, viz., (1) The North Kootenay Pass; (2) The Crows Nest line of the Canadian Pacific Railway; (3) The main line of the Canadian Pacific Railway.

The North Kootenay Pass crosses the Rocky Mountains about 20 miles north of the international boundary line. A geological reconnaissance along this line was made by G. M. Dawson, when Geologist to the Commission appointed to establish the international boundary on the 49th parallel. A great development of Devono-Carboniferous rocks is shown in Dawson's section, both to the east and the west of the Flathead Valley. To the east a block of these strata is represented as being thrown down between two faults, this faulted block being capped by a series of 'red beds' regarded by Dawson as of Triassic or Permian age, having been correlated by him with a similar series of beds found by Meek in Utah. If this correlation is correct a phosphate-bearing horizon might be expected a short distance below the base of the 'red beds' in question. An examination of this district, however, showed that only one fault existed and that the section contained no rocks of Carboniferous age. A thick series of Devonian limestones directly overlie a thinner series of orange limestones and shales in which was discovered an abundant trilobite and brachiopod fauna (the Albertella fauna), which fixes the age of these strata as belonging to an horizon near the base of the middle Cambrian. The underlying quartzites 'red beds' and limestones, are, therefore, of lower Cambrian or Pre-Cambrian age. The proper horizon for the phosphate of lime does not exist, therefore, in this portion of the North Kootenay Pass.

An examination of the Crows Nest Pass section shows that here on Turtle Mountain only Devonian or Lower Carboniferous limestones are present, and these are directly overlain by a strata of Jurassic age.

In the third section, that on the main line of the Canadian Pacific Railway where this crosses the Rocky Mountains Park near Banff, the Carboniferous occurs in great volume and with a development very similar to that in Montana. There is the fourfold division as shown

by Allan who has mapped this area for the Geological Survey of Canada. From stratigraphical and paleontological considerations the authors were led to the conclusion that the horizon equivalent to the Phosphoria formation of the south was that at the contact between the Rocky Mountain quartzite and the Upper Banff limestone of Allan's section. These formations are repeated by faulting and appear as a set of parallel bands which cross the area four times in a general northeast and southwest direction. Forty Mile Brook crosses the contact three times, and a search was accordingly made in the shingle and loose blocks carried down by the stream, for 'float' of phosphate rock. This was found in the stream at the foot of Stony Squaw Mountain. The phosphate rock is of a hard compact variety which somewhat resembles basalt, a type found in certain of the Montana deposits. It contains 54% of tricalcic phosphate. An examination of the mountain itself showed that a heavy band of chert occurs along the contact of the Rocky Mountain quartzite and the Upper Banff limestone, and a chemical examination showed that this chert in all cases carries phosphoric acid in small amount. A thin bed of phosphate of lime was also found on the mountain in association with this chert.

The discovery of this phosphate rock was reported to the director of the Geological Survey of Canada and Mr. DeSchmidt was directed to make a detailed examination of the phosphate bearing horizon for the purpose of ascertaining whether beds of commercial value could be discovered. This gentleman had discovered two beds of which the larger was two feet in thickness, when his examination was brought to a close by the snowfall. The search, however, will be continued by the Geological Survey of Canada next spring.